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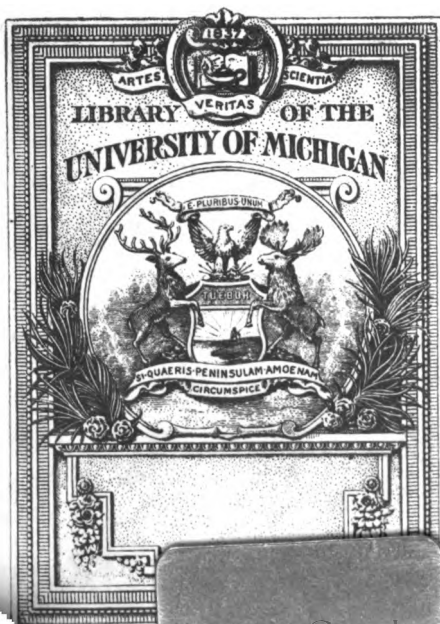
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HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.
JANUARY—JUNE,
1863.

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THE
HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES:

BEING

A PRACTICAL AND ANALYTICAL DIGEST OF THE CONTENTS OF THE PRINCIPAL
BRITISH AND CONTINENTAL MEDICAL WORKS PUBLISHED
IN THE PRECEDING SIX MONTHS :

TOGETHER WITH A

SERIES OF CRITICAL REPORTS ON THE PROGRESS OF MEDICINE AND
THE COLLATERAL SCIENCES DURING THE SAME PERIOD.

EDITED BY

W. H. RANKING, M.D. CANTAB.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS OF LONDON, PHYSICIAN TO THE WORFOLK AND
NORWICH HOSPITAL;

AND

C. B. RADCLIFFE, M.D. LOND.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS OF LONDON,
PHYSICIAN TO THE WESTMINSTER HOSPITAL, AND LECTURER ON MATERIA MEDICA AT THE WESTMINSTER
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Dublin Hospital Gazette.
Edinburgh Medical Journal.
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HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

ETC.

PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE.

(A) CONCERNING HYGIENE.

ART. 1.—*Air and Water: their Impurities and Purification.*

By Mr. HENRY BOLLMANN CONDY.

(8vo. London, Edinburgh, and Dublin: Davies, MacLachlan & Stewart, and Fannin & Co. pp. 80. 1862.)

THE real object of this work is to recommend a solution of permanganate of soda or potass, which has been patented by Mr. Condy under the name of "Condy's Fluid," as a deodorizer and disinfectant, and this object is carried out in a very satisfactory manner. Indeed, the only exception we have to take is with the title, which is much too vague to answer its purpose fully.

In the article on Disinfection in *Ure's Dictionary of Arts, Manufactures, and Mines*, the writer says:—"One of the most thorough methods of oxidization is by the use of the manganates or permanganates. They transfer their oxygen to organic substances with great rapidity, and completely destroy them. They are, therefore, complete disinfectants. They destroy the odour of putrid matter rapidly, and oxidize sulphuretted and phosphoretted hydrogen as well as purely organic substances. As they do this by oxidization at a low temperature, they are the mildest form of the destructive disinfectants, and their application to putrid liquids of every kind gives the most satisfactory results."

Liebig also, writing to Mr. Condy, says:—"I have myself made a series of experiments which have convinced me that your statements about the excellent effects of your disinfecting fluid in destroying bad smells, as well as of unwholesome contaminations to which

drinking water is subject, are perfectly correct. It has proved itself of very great service for purifying the mouth, and washing the feet and other parts of the person. I consider it an inestimable means for the preservation of the health." And this strong testimony is corroborated by many other testimonials of a similar character from other chemists.

It is easy to see that Condyl's Fluid is likely to have other great uses besides purifying foul water and air and correcting tainted meat. Already it has been used extensively for surgical purposes, as to destroy the odour of offensive discharges, and in this respect it seems to have no worthy rival. Nor are its uses likely to be confined to surgery. In a word, we regard the use of this fluid as a great fact, and we recommend it to our readers, together with the book in which an account of it is given.

As illustrating some of the more interesting properties of the alkaline permanganates (Condyl's Fluid), our author gives in an appendix certain simple experiments, of which the following may be taken as a sample:—

To test water for organic impurities.

"Take any number of tumblers; fill up one with distilled water, another with ordinary drinking water from a pump, rain-water butt, or other supply, and the rest with various samples of water more or less contaminated with organic impurities, such as sewage water. Add to each of them, drop by drop, Condyl's Fluid (crimson) till the contents begin to assume a decidedly pink hue. This effect will be produced, in the case of the distilled water, if pure, by a single drop; more will be required by the drinking water, which, after standing a little while, will show some signs of muddiness, and a still larger portion by the other samples, in which a brown precipitate will soon form. The quantity of fluid required, and the amount of muddiness produced in each, will be the measure of the relative impurity of the several waters. This experiment may be repeated with other deodorizing fluids, and the results contrasted with those produced by Condyl's. By having a separate set of glasses for each of the deodorizers to be tried against one another, the comparison will of course be more conclusive.

To prove the permanent nature of the action of Condyl's Fluid.

"To the above, or other similar samples of water, after treatment with the various deodorizing agents, add a few drops of sulphuric acid; in those in which chloride of zinc or other such deodorizer has been used, any offensive odour which they previously had will be immediately revived, whereas no such effect will follow on adding the acid to those waters that have been treated with Condyl's Fluid.

To purify water on a practical scale.

"Pour into a hogshead of offensive drinking water one wine-glassful of Condyl's Fluid, and mix with a stick or lath. Generally this quantity will render it as sweet as fresh water; should it require more, add half a wine-glassful. So long as organic matter remains—

which is known by the pink colour of the fluid gradually vanishing—add the fluid. If a trace too much has been used, continue stirring, or immerse a stick or lath, and the colour will disappear. Let it stand, and any suspended matter present will subside or filter.

“The purifying action of Condyl's Fluid on water will appear very evident on carefully comparing a sample of common drinking water, after its treatment by the fluid, with another of the same water taken previously to being treated. Filter both through clean blotting-paper, and let a person taste them alternately blindfold, or without being aware which of them was the one that had been operated upon. A peculiar purity of taste and smell which characterizes the purified water, and can be compared to nothing so well as to the fresh dew of the country, will cause the two samples to be very soon distinguished from each other. In most instances a marked difference of colour will also be perceptible on placing a glass of each on a sheet of white paper, and looking through them upon it: the purified water will generally appear more colourless and brilliant than the other.

To prove the superiority of Condyl's Fluid for the purification of water over filtration through charcoal, which is generally considered the best known means of purifying water.

“Macerate some hay in a jug of water in a warm place until the contents are charged with vegetable matter: strain and pass through a charcoal filter. Operate on the water so treated with Condyl's Fluid, as in the above experiment, when it will be clearly shown that, notwithstanding the use of the charcoal, it still contains a considerable quantity of organic matter.

To demonstrate the innocuous nature of Condyl's Fluid as compared with Chloride of Lime.

“Take two flower-pots, containing each a plant of mignonette, or other common household flower; water them both during several days, one with water to which Condyl's Fluid has been added in the proportion of a teaspoonful to a pint of water, and the other with water into which the same quantity of chloride of lime has been thrown. Examine them from day to day, and in no very long lapse of time the latter will be found drooping or dead, while the former will be as flourishing as ever, or even more vigorous in its growth.

To show the poisonous properties of Chloride of Zinc as compared with those of Condyl's Fluid.

“Have two ordinary glass fish-globes, each filled with water, and containing some common kind of fish, which can be easily procured; mark one globe B and the other C. At the time when the daily change of water is made, add to the globe B ten drops of Burnett's Fluid for every gallon of water, and to C Condyl's Fluid in the same proportion; continue this treatment daily, taking care always to give Burnett to B and Condyl to C, till such time as the fish in the former turns on his back, which will very soon be the case. However often this experiment is repeated, and with whatever proportion of fluid,

the fish B will uniformly die first, whereas C will in many instances improve in health and condition.

"The result will be the same, when, instead of chloride of zinc, other deodorizing agents are tried in this manner against Condyl's Fluid; chloride of lime, quicklime, perchloride of iron, tar-water, and those numerous antiseptic fluids which were formerly so generally but fallaciously relied on as disinfectants, all exert most injurious effects on animal and vegetable life.

*To exhibit the efficacy of Condyl's Fluid in removing taint
from meat.*

"Take a small piece of fish, beef, or other flesh, place it on a plate, and covering it over with an inverted tumbler, there leave it. When it has acquired a decided taint, remove it, and after making several incisions, immerse it in a solution of one table-spoonful of Condyl's Fluid in a quart of water. Allow it to remain for about ten minutes, after which it may be removed and washed with pure water, when it will be found to be free from taint."

ART. 2.—*On Malaria and Miasmata, and their Influence in the Production of Typhus and Typhoid Fevers, Cholera, and the Exanthemata: founded on the Fothergillian Prize Essay for 1859.*

By Dr. T. HERBERT BARKER.

(8vo. London, Edinburgh, and Dublin: Davies, MacLachlan & Stewart, and Fannin & Co. 1863. pp. 251.)

Dr. Barker is an earnest and successful pathologist, and the present work ought to add not a little to his reputation. At any rate, it is by works such as this, where original experimental investigation enters as a conspicuous element, that sound theories are in the end most likely to be educed. Dr. Barker's principal conclusions are these:—

"1. I define as a malarious agent, any agent, whether organic or inorganic, which, being so diffused through the air as to admit of being inhaled, possesses the power of exciting specific symptoms in the person who inhales it.

"2. The diseases placed before me for consideration, viz., typhus and typhoid fevers, cholera, and the exanthemata, have each their origin in a specific poison which is organic.

"3. The fact of the organic nature of the poisons is proved by their power of reproduction, their communicability from person to person, their destruction at a temperature extremely high, and the suspension of their activity as poisons at a temperature extremely low.

"4. Meteorological changes alone are quite insufficient to account for the origin and spread of the abovenamed diseases. But meteorological peculiarities may affect the course of those diseases by exerting an influence on the organic poison. A low temperature may

arrest the poison for a time ; a warm temperature with moisture may, and in all probability does, favour the reproduction of those poisons, whatever may be their nidus, the bodies of men, or the earth, or water. Again, when the thermometer is high and the atmospheric pressure is diminished, the diffusion of malarious products is increased. Thus on the eve of rain we all know that disagreeable emanations from cesspools are most distinguishable. The vapours producing the bad odour are at such times rapidly evolved, and with them organic matters which may or may not be poisonous, but which being poisonous are then readily diffused.*

"5. The organic poisons which excite the diseases under our consideration obey the same laws as regards generation and propagation as do other and more complicated representatives of the organic series. Under favouring conditions to each successive act, they arise, reproduce, and die.

"6. These organic poisons, infinitely minute in themselves, reach the bodies of men through two sources—through the air taken in by the lungs—through the matters taken in by the mouth. With water at ordinary temperatures as their medium, they are undestroyed, in the air they are undestroyed, in any medium they are undestroyed if the temperature and chemical character of such medium is compatible with the persistence of organic life.

"7. Some of the organic poisons, the poison of cholera for example, seem mainly communicated to man by the alimentary surface, and travel but small distances by the air. They are, however, transmissible through either of the mucous surfaces, the position being only allowed, that they *reach* the mucous surface and alight upon it. The poison of typhoid seems communicable both through air and water : the poisons of the exanthemata seem mainly to be transmitted by the air.

"8. In filthy localities, in cesspools, in sewers, in decomposing organic remains, other poisons inorganic in character are generated. These poisons are capable of producing certain specific symptoms, analogous in many points to symptoms caused by the organic poisons. Thus sulphuretted hydrogen produces intestinal disorder and prostration ; while sulphide of ammonium sets up a class of symptoms resembling closely those of typhoid fever.

"9. These inorganic poisons are not competent for the production of communicable disease, the symptoms they produce being confined to the body in which they (the symptoms) are demonstrated.

"10. The connexion which exists between the organic and the inorganic poisons is the connexion of coincidence. Both find the same localities the most favourable points for development, and the two often go side by side.

* "At the same time sudden changes in the temperature and pressure of the air may for the moment produce an effect on human bodies ; for instance, a sudden and marked fall in the temperature may produce amongst people unprovided for the change a general attack of diarrhoea. But this class of disorder does not assume a continued epidemic character, never becomes communicable, and is not usually fatal in its issues."

"11. Thus, while the specific actions of these two classes of poisons must not be confounded together, their possible connexion must not be overlooked. The presence of the inorganic poison in the person of the sufferer may materially intensify the action of the organic poison, and even dictate the type of the disorder. As 0·051 per cent. of sulphuretted hydrogen diffused through the air, a proportion imperceptible to the senses, will affect a healthy animal, sicken it, and prostrate it, it is a self-evident proposition that the same agent present in the air, and breathed by the fever-stricken person, will intensify his symptoms and influence their course.

"12. While it is impossible that an inorganic poison can ever be the prime source or generator of organic poisons producing specific disease, it is certain that in the course of certain systemic diseases (such as peritonitis) which are not considered communicable, organic products may be formed, which products, conveyed to susceptible persons, may communicate at least the disease puerperal fever, which in its turn is communicable."

ART. 3.—*Statistics of the Metropolitan General Hospitals.*

By Dr. —.

(*Medical Times and Gazette*, September 13, 1863.)

The *Statistical Journal* for August, 1862, contains a series of tables, compiled from the returns of the various hospitals, which, although not so perfect in detail as could be wished, constitutes a very acceptable instalment of information. The first thing which strikes the reader of these tables is the enormous aggregate of disease and injury treated in the wards of the London hospitals. The total number of in-patients during the year 1861 was 35,506. This, which surpasses the number of the entire population of many a provincial city, and exceeds that of one English county, it will be remembered represents only cases of serious illness; none others, as a rule, obtain admission as in-patients. Of these 3065 remained in hospital on January 1, 1862, 3131 died, 884 were discharged for special reasons, 929 were sent out unrelieved, 7561 relieved, and 19,950 were dismissed well or convalescent. The average rate of mortality was 9·5 per cent. If we classify the London hospitals according to their size, we find six—St. Bartholomew's, Guy's, St. Thomas's, the London, St. George's, and the Middlesex—admitting each annually upwards of 2000 patients; the maximum return being made by St. Bartholomew's, 6124—the minimum by the Middlesex, 2265. The next group contains St. Mary's, the Westminster, King's College, the University, the Royal Free, and Charing-cross; in them the number of in-patient admissions ranges between 1000 and 2000. Of these the maximum is furnished by St. Mary's, 1822—the minimum by Charing-cross, 1023. In the third group are two small hospitals—the Metropolitan Free and Great Northern: the number of in-patients admitted at these last year was respectively 154 and 150. Subjoined is the death-rate for 1861 in each:—

	Per cent.
St. Bartholomew's	10·7
Guy's	9·4
St. Thomas's	9·7
London	8·4
St. George's	8·3
Middlesex	11·7
St. Mary's	10·1
Westminster	9·6
King's College	10·7
University	11·2
Royal Free	6·0
Charing-cross	8·3
Metropolitan Free	6·8
Great Northern	8·2

It will be seen from these figures that the size of the hospital appears to exercise but a minor influence in determining the rate of death. The highest mortality took place amongst the 2265 patients in the Middlesex and the 1386 in the University. The death-rate of the 5360 patients at Guy's was slightly below the average, whilst that which occurred in the huge sick population at St. Bartholomew's was no higher than that of the 1452 patients at King's College. It is true that the instances of the smallest mortality are to be found in the returns of the small hospitals, but the deaths at the London and St. George's—the former with 4520 patients, the latter with 3981—are both below the mean standard. The other causes which have influenced the rate of death can only be a matter of speculation, as the tables furnish no information as to the nature of the diseases and injuries, the hygienic conditions, or the general character of treatment adopted. The returns, however, enable us to contrast the relative fatality of medical and surgical diseases as seen in the wards of the London hospitals. The total number of cases in the medical wards in 1861 was 13,346; the total in the surgical, 18,236. It is a remarkable fact that the death-rate of the former was more than double that of the latter. The average mortality in the medical wards was 12·9 per cent., that in the surgical 5·6; an apt illustration this of the greater fatality of diseases of the internal organs than of accidents to and diseases of the periphery. The per-centage of deaths amongst medical cases shows the greatest variation. The largest average is that of St. Bartholomew's, 18·7 per cent.; the smallest that at the Metropolitan Free, 7·9. St. George's and the Royal Free are also both low; the former 10·9, the latter 10·5. The highest mortality amongst surgical cases occurred at St. Mary's, King's College, and University College hospitals; the per-centage in these hospitals being 8·5, 8·2, and 8·0 respectively. The smallest number of deaths amongst surgical cases is returned by the Royal Free, 4·8 per cent. In the three great hospitals, St. Bartholomew's, Guy's, and St. Thomas's, the mortality of surgical cases is nearly identical, viz.:—St. Bartholomew's, 5·6; Guy's, 5·6; and St. Thomas's, 5·5 per cent. Surgical cases are kept longer in hospital than medical; the average for the

former is thirty-five days, for the latter thirty-one. The great endowed hospitals of St. Thomas and St. Bartholomew afford the longest shelter to their patients. The mean residence of surgical cases at St. Thomas's is fifty days, of medical cases thirty-nine; at St. Bartholomew's the mean residence of surgical cases is forty-four days, of medical thirty-six.

ART. 4.—*On the Selection of Patients as affecting the Mortality in Hospitals.*

By Dr. GUY, Physician to King's College Hospital, &c.

(*Medico-Chirurgical Review*, January, 1863.)

In an original communication "On the Rate of Mortality prevailing in the General Hospitals of the Metropolis, and the Causes by which it is brought about," Dr. Guy says:—

"Seeing that the rate of mortality in hospitals varies so little with locality, site, size, and spaciousness, and so much with the sex of patients, and the medical and surgical character of their maladies, it is obvious that more may be done to raise or lower the rate of mortality of our hospitals by selection of patients than by all other causes put together, excepting always such a degree of over-crowding and neglect of obvious sanitary precautions as are no longer possible in the hospitals of London. It must also be quite evident, that if to an unequal distribution of men and women, and a variable proportion of medical and surgical cases, we were to add the element of a selection by one hospital of the more serious class of cases, medical and surgical, and by another of cases of a less severe character, we should be able to produce at will almost any rate of mortality between the limits of 50 or 60 per 1000 and 110 or 120 per 1000. Now this sort of selection does go on almost unconsciously in the case of those hospitals which have attached to them medical schools, and in the greatest degree in those which have the largest schools, and through them the largest connexion of old students. The attendance of a numerous class of pupils craving for instruction leads naturally and necessarily to a selection of severe and dangerous cases, while the attachment of practitioners to their *alma mater* brings about with equal certainty a supply of cases for medical treatment and for surgical operation, among which cases an undue mortality may be expected to occur. That this is the true explanation of the higher rates of mortality to be found in the tables published by the Statistical Society, is rendered in the highest degree probable by the fact that the four hospitals which present the highest rates of mortality are St. Bartholomew's and Guy's, King's College and University College. In the last of the series of published tables the mortality of these four hospital schools is shown to range from 110 to 115 in 1000. The other hospital schools are found to occupy an intermediate position between the four hospitals which have the honourable distinction of a higher rate of mortality and the hospitals which have no medical schools attached, and which enjoy the unenviable privilege of displaying a rate of mortality rising from 60 to 82 in 1000.

"The writer of this paper hopes to have an early and more suitable opportunity of establishing by a more considerable array of figures the important fact that the great leading cause which determines the mortality of hospitals is the *selection of patients*—a cause which, at the point of sanitary excellence our London hospitals have now attained, appears to him to be the real determining cause of a high or a low death-rate."

The whole paper is well worthy of attentive perusal, and particularly that part of it which cautions us against rash attempts to compare the rates of mortality of the hospitals of London and Paris, and to draw from such comparisons inferences unfavourable to the construction, arrangement, and management of the hospitals of either capital.

ART. 5.—*On the Sickness and Mortality of the United States Army.*

By Mr. ELLIOTT and Mr. WOODWARD.

(*American Medical Times*, April 4, 1863.)

Every one who has ever been interested in sanitary science will look forward with eagerness to the official reports of the sickness and mortality of the vast armies of the United States. The only data which have yet been published upon which an opinion can be based are embraced in the report of Mr. Elliott, Actuary to the Sanitary Commission, and in the report of Assistant Surgeon Woodward, to the Surgeon-General. The latter report gives definite returns for each disease for the year ending June 30, 1862. Mr. Elliott's calculations are more general, but not the less valuable, while the comparisons which he has instituted are of great interest.

"The general mortality of the army has been, according to Mr. Elliott, at the annual rate of 53·2 per 1000 numerical strength, of which 44·6 were from disease and accident, and 8·6 from wounds received in action. Assist. Surgeon Woodward gives the mortality for 1862, exclusive of wounds, at 65 per 1000. These statistics in either case compare most favourably with those of the British army. During war, from 1803 to 1812, its annual death-rate was 80 per 1000, 71 per 1000 being from disease and accident, and 9 from wounds in action. In the Peninsular war, 1811–14, the annual death-rate was 165 per 1000, of which 113 were from disease and accident, and 52 from wounds received in action. When, however, we contrast the mortality of our army with that of the English during the Crimean war, its mortality becomes insignificant. The annual death-rate of the British army was fixed at the frightful figure of 232 per 1000, or about four and one third times that of our volunteer army. The mortality of our army during the present war compares most favourably with its mortality during the Mexican war. The death-rate then was 118 per 1000 per annum, or more than double the rate of the war of the rebellion during its first year. It is apparent that the general mortality of the U.S. army forces is comparatively very small. The ratio of mortality from diseases and

from wounds is, as usual, markedly in favour of the former. The mortality from diseases was five-sixths for the men, and two-thirds for the officers. In the Crimean war seven-eighths of the mortality of the British troops were from diseases. The difference in the causes of death among officers and privates is noticeable. The liability of the private to die of disease is double that of the officer, while his liability to die of wounds is only one-third as great.

"Other deductions are made in Mr. Elliott's interesting report. The mortality by seasons is exhibited as follows:—For the summer months 21 per 1000, for the fall months 36 per 1000, and for the winter months 70 per 1000. This apparent gradual increase of the mortality is attributed in part to the greater completeness of the returns as the war progressed. The mortality by localities where recruited and by rank is also noted as follows:—The annual death-rate of the troops recruited in the New England States was 36 per 1000 strength, of which 31 was from disease and accident, and 5 from wounds in action; the rate for commissioned officers being 16, and for enlisted men 37 per 1000. For the middle State Group (including with the Middle States proper, the States of Delaware, Maryland, and Virginia) the annual death-rate was 30 per 1000 strength, of which 27 were from disease, &c., and 3 from wounds in action; the rate for commissioned officers being 22 per 1000, and for enlisted men 30 per 1000. The annual death-rate for the New England and Middle Groups combined was 32 per 1000 strength, of which 28 was from disease and accident, and 4 from wounds in action; the rate for commissioned officers being 21, and for enlisted men 32 per 1000. The rate for the Western Group (including, with the Western States proper, the States of the Southwest) was 95 per 1000 per annum, of which 77 were from disease and accident, and 18 from wounds in action; the rate for commissioned officers being 59, and for enlisted men 96 per 1000. The mortality of the armies recruited at the West (and which as a rule operate at the West) is almost precisely three times that of the troops recruited in the Middle and New England States (and which as a rule serve with the armies at the East), the Western rate from wounds received in action being five times; and that from disease and accident a little less than three times as great as the corresponding Eastern rates.

"The sickness rates in the Eastern and Western Departments present the same contrasts. Mr. Elliott remarks:—'There is reason for the belief that the excess of the rates of sickness and mortality in our Western armies over those in the East is due, in no small degree, not merely to the greater activity of the former in the field, to over-exertion and exposure as the result of severe and long continued marches, and to stubborn and deadly encounters with the enemy in arms, but also to badly chosen camp sites, to imperfect and neglected drainage (the nature of the surface and soil not unfrequently being such that suitable camp sites, free from malaria, and affording ample facilities for drainage, could not be found if sought); to the too crowded condition of hospitals; to less of variety in food (soft bread and desiccated vegetables in very many Western regiments being seldom or never had), and to less of skill and care

in its preparation; to water of impure quality and sometimes of insufficient quantity; to the greater disposition on the part of the soldiers to neglect appliances for personal comfort; and to the greater neglect of, or a lack of means for, enforcing cleanliness of person and camp.

"The valuable statistical tables of Assistant-Surgeon Woodward will interest the student of military hygiene. The zymotic diseases, as might have been anticipated, greatly predominate. We have no space to dwell on the interesting questions which these tables suggest. Large as is the aggregate of sickness and mortality in our army, so far from being excessive, it is less than that of European armies."

ART. 6.—*Diseases of Animals in Relation to Public Health and Prosperity.*

By Mr. GAMGEE, of Edinburgh.

(*Medical Times and Gazette*, April 25, 1863.)

In this paper, which was read at a recent meeting of the Metropolitan Associate of Officers of Health, Mr. Gamgee considers the sale of diseased meat as it affects the health of stock, the health of the people, and the wealth of the dealer. It is, he argues, to be put down on all these grounds. In speaking on the first topic, there is, he says, practically little difficulty with indigenous diseases, because the amount of stock destroyed by them is limited, and because the diseases themselves are greatly under the control of proper hygienic measures. But with imported disease it is different. "A cargo of Dutch or German beasts arrives from Rotterdam or Hamburg. The whole stock is only fit for slaughter, and it enters our ports with signs of the foot and mouth disease. These animals are exposed with British stock in our markets, grazed in our fields, and soon show signs of pleuropneumonia. We kill them out, but our stock has been seized; the diseased continue to be slaughtered, but are also permitted to travel on roads, in railway-trucks, in steamers, and to be handled by butchers in the public markets amongst animals for store purposes, and dairy cows that are to supply our children with milk. These animals in their turn become affected, and the number increases in geometrical proportion." Mr. Gamgee considers that the energetic measures adopted in the epidemic of small-pox in sheep in Wiltshire have prevented that disease from spreading "throughout the length and breadth of the land." But with cattle it is very difficult to persuade the farmer to make an early sacrifice in order to get rid of a contagious disease. It seems to him a reckless extravagance to bury or turn to manure the carcass of a diseased animal which it is possible to sell at an auction of live stock or at market. Thus:—"The farmer's practice now is to generate disease and sell diseased stock." Again:—"Rather than spend 5*l.* in professional advice, he would lose 500*l.* by disease." Mr. Gamgee does not think that veterinary surgeons can do much when the disease is raging; but a vigorous and talented body of men ought "to prevent damage, to

smother flames, and attend to the property in the neighbourhood." To prevent contagion, and not to drug individual cases of the disease, should be the surgeon's object. Mr. Gamgee insists very strongly on the contagious nature of the foot and mouth disease, and of pleuro-pneumonia. In support of this view he has recently obtained a large mass of evidence in replies to a circular he had sent out to 300 veterinary surgeons. The fact that these diseases rarely appear in breeding districts, but affect the neighbourhoods of markets and lines of traffic, shows that this opinion is correct. In speaking of the effect of the sale of diseased meat on the health of the community, Mr. Gamgee first alludes to the diseases of the pig. It is well known, he said, "that it is not only the irritating tapeworm that we derive from the pig, it is the prostrating and destructive trichina, which penetrates our muscles and kills. Probably there are many more cases of trichina disease than of tapeworm, and the day is not far distant when the number of well-recorded cases of death due to trichina in man will convince every one that the meat-shops, slaughter-houses, and bacon-factories must be looked after to stop the sale of dangerous bacon just as much as we require to analyse for the adulteration of groceries.

"I am, however, quite convinced that the tens of thousands of carcasses of diseased animals sold in all large towns are stealing life from human beings when and where we least expect it. It is asserted by many at home and abroad that the flesh of cattle affected with pleuro-pneumonia is wholesome. I hope the day will soon arrive when we need scarcely discuss such a question in England, and, if Government wishes, this can be attained; but, as the disease is here, I must say that last year my opinion became confirmed that the flesh of cattle affected with pleuro-pneumonia when eaten by man induces boils and carbuncles to an incredible extent. My observations have been made in three establishments. One where 1500 men are known to be supplied, fraudulently of course, with little else than diseased meat; another where several hundred soldiers are in the same position; and a third where about seventy individuals fed, too often to my knowledge, on the flesh of cattle affected with pleuro-pneumonia, have been seized occasionally with vomiting, diarrhoea, abdominal pains, &c., and have traced such accidents to the meat, to such an extent that many refused to eat it."

Mr. Gamgee speaks also, and adduces examples, of the injury produced by eating the flesh of animals which had died of "splenic apoplexy," "parturition fever," and "foot and mouth" disease; and after this he refers to the loss to the town dairyman, to the breeder of cattle, and to the community. Dealers, he says, have looked on him as their worst enemy, whereas they would, he had little doubt, find in time that he was their best friend, as he was sure that free trade in live stock, without due regard to the prevention of contagious diseases, "had been the curse of this stock-producing country since 1842." It was a great mistake to suppose, as some do, that the poor gained anything, even in the amount of food. It was much higher in price since the importation of foreign stock, and, at the same time, of foreign contagious diseases.

"Beef, in 1841, sold at from 3s. 8d. to 5s. the stone. It now sells at from 5s. 4d. to 8s. 10d.; and when you consider that the amount consumed in the United Kingdom is estimated at 80,000,000 stones, if we take the increase in price during the last twenty-two years, in round numbers, at 2s. 6d. a stone, the meat-consuming public is paying 10,000,000*l.* more now for the same amount of meat than it did in 1841."

Mr. Gamgee considers that this injury to the public health and waste of the national resources might be put a stop to with certainty, not by the action of individuals, but by sound legislation on the subject of the importation of diseased cattle. The present systems of inspecting imported cattle and of inspecting markets are extremely inefficient. The paper advocates the appointment of a Government Inspector, who, while making rules for the control of the trade in foreign stock, and for the stricter supervision of markets, might become the centre towards which information should converge, and from whose experience advice for the whole country might proceed.

ART. 7.—*Sanitary Laws and Sewage Irrigation among the Ancient Jews.*

By Dr. —.

(*Medical Times and Gazette*, February 28, 1863.)

It is well known that most of the ceremonial laws of the Jews have a profound philosophical and sanitary import, apart from their religious significance. The rite of circumcision, which was adopted not only by the Jews, but by the Arabians, Egyptians, and many of the tribes which migrated along the eastern coast of Africa, is so important as a means of cleanliness, of preventing certain sexual impurities, and of averting disease, that many of us wish it had never been abolished amongst Christians; and though it may be said that it is unnecessary for a cleanly and moral people, yet, alas! Christians are far from being cleanly and moral yet. The prohibition of pork, and of the flesh of animals which had died of themselves, was well calculated to prevent the spread of disease arising from the parasitic animalcules with which filthily-kept pigs are infested; but as it is possible to keep those useful creatures in a cleanly and wholesome manner, so we may claim the privilege of eating pork. The absolute prohibition of blood is a thing more difficult to understand. We can conceive of the expediency of a law against eating "flesh with the life"—*i.e.*, the flesh of animals cut from them whilst living, after the disgustingly cruel fashion described by Bruce in his Abyssinian travels; but we feel that there is something yet to be learned of the reasons for that primeval law against eating blood, which was given to Noah, confirmed by Moses, and singularly enough sanctioned by the council of the Apostles at Jerusalem. In the Eastern Church this law is, we believe, observed to the present day; and neither blood, nor the flesh of animals which have died of disease or by strangulation, is permitted to be eaten. In the Western Church it fell into disuse about the sixth century, and blood became

not only a common but a popular article of food, and continued so over Western Europe until a more delicate style of eating was introduced in the present century. We have a cookery-book of the year 1780, by C. Carter, cook to George III., and amongst the bills of fare for a grand dinner in October, we find "a Beef-Pye in Blood;" and that "a Haunch of Venison, Roast in Blood," figured on his Majesty's table on December 20th, in some year not mentioned. The blood was that of sheep, calf, lamb, or deer, mixed with a little salt, stirred to hinder "clodding," and rendered savoury by spices and herbs. The meat was immersed in this all night; and in the case of a pie the dish was filled up with the blood, and in the case of a roast the meat, blood, and all were done up in a veal caul, and so roasted altogether. There is evidently room for a revival of archaeological cookery, if any one is tired of modern dinners. But to return to the sanitary customs of the Jews. We were not aware until Mr. Williams gave his lecture on Jerusalem, last Friday, at the Royal Institution, that the practice of sewage irrigation was known to them. But so it was.

"Recent discoveries," said Mr. Williams, "fix beyond all question the position of the temple of Solomon and its successors. No one could read the accounts of the sacrifices without being convinced that there must have been some great system of water-works for the purpose of clearing away the blood and other impurities resulting from them. There is a hole in the rock which Professor Willis was the first to identify with its real purpose, and underneath the 'dome of the rock' Signor Pierotti discovered a cave. The hole was the drain, and the cave in the rock the cesspool of the Jewish altar. This confirms the position of the altar of burnt-offering.

"The water came from the Pools of Solomon, which exist to this day, and passing in an easterly direction turned northwards, and flowed under the dome of the rock. But we are told in the Mishna that the place of slaughter was on the north; and accordingly we find two cisterns, one at the north and the other on the west, from which aqueducts led eastwards and southwards to carry away the blood and offal from the altar. A further dilution was made by the accession of water from the Pool of Bethesda on the north, whence the mixture passed on to the valley of the Kedron, where it was sold to the gardeners, and the lands manured with it were remarkable for their fertility."

If we understand the subjoined notes from the Mishna aright, the sin-offerings were held not to have been duly performed till the blood reached the brook Kedron—*i.e.*, the extremity of the outfall sewer, and was delivered to the gardeners. Our Metropolitan Board of Works may take a lesson in this matter from the wisdom of the ancient Jews.

"*Super locum mundum altaris spargebat septies, reliquiasque sanguinis in lembum occidentalem altaris exterioris fundebat, reliquias vero sanguinis altaris exterioris fundebat in lembum ejus meridionalem. In fossa autem miscebantur, et currebant in torrentem Cedron, et stercorandis hortis olitoribus vendebantur, et oblationem transgressionis persolvere tenebantur in illis.*

"*Note.*—Isti et isti in fossa miscebantur et currebant in torrentem Kedron. Isti et isti, id est, sanguines interiores et exteriores miscebantur. Duo enim foramina erant in basi altaris, instar duarum narium exilium, ut in '*Middoth*' traditur, ex quibus utrinque sanguines in fossam subterraneam cadentes, simul in torrentem Kedron ferebantur.

"*Note.*—(*Et Stercorandis—in illis.*) Sanguis autem iste ià Tribunis ærariis et rei quæstuariæ præpositis vendebatur, nec oblationem transgressionis persolvere tenebatur, antequàm sanguis esset in torrentem Kedron devolutus."

ART. 8.—*A Simple Means of Removing the Poisonous Properties of Suspicious Mushrooms.*

By M. GÉRARD.

(*Journ. de Méd. et Chir. Prat.*, Novembre 15, 1862.)

We reproduce from the columns of the *Journal des Connaissances Médicales Pratiques* a few extracts which seem to be deserving of attention:—

"By repeated experiments on *twelve persons*," says M. Gérard, "I have ascertained all the means by which mushrooms of the most poisonous description can be rendered wholesome, and I fear no contradiction. I gather the agarics without choice, and without bestowing a thought on any other points but their sufficient firmness and size.

"In the course of one month I gathered upwards of one hundred and fifty pounds of poisonous mushrooms, such as *Agaricus muscarius*, *A. emeticus* and *sanguineus*, *Boletus luridus*, *B. aurantiacus*. For a week, in spite of the repugnancy induced by uniformity of food, I resolutely eat twice a day more than half a pound of these boiled fungi.

"As I experienced no evil effects from their consumption, and suspecting that my personal susceptibility to vegetable poisons might possibly have been blunted by frequent experiment, I caused all the members of my family, twelve in number, to join me in my researches. No ill effects followed—the test was clearly decisive; and, satisfied that I had perfectly succeeded, I endeavoured to discover the precise amount of time and of liquid necessary to convert the most poisonous mushrooms into innocuous nutriment. I came to the following conclusions, which have now been repeated so often as utterly to defy contradiction:—

"Every pound of mushrooms, cut into moderately small pieces (in four for middle-sized agarics, in eight for larger varieties), requires two pints of water, acidulated with an ounce or an ounce and a half of vinegar, or containing two table-spoonsful of bay-salt. The mushrooms should macerate for two full hours in this fluid, be well washed, and subsequently boiled in water for twenty minutes or half an hour; they should then be taken from the fire, again repeatedly washed, dried, and served either alone or as a condiment."

M. Gérard, it will be remarked, does not recommend vinegar alone, but also salt, as likely to effect the desired purpose.

ART. 9.—*An Argument in favour of Maternal Breast-Nursing.*

By Dr. J. WHITEHEAD.

(*Medical Times and Gazette*, February 7, 1863.)

In some admirable and elaborate notes on the causes of early mortality, Dr. Whitehead says:—

“The immense preponderance of deaths during the first year, of those born out of wedlock, is painful to contemplate, and serves to illustrate how largely the life and well-being of the infant are dependent upon the natural sustenance and care which its own parent alone has the power of imparting. Any other form of aliment, however judiciously prepared, even cow's milk of faultless quality, however scientifically adjusted by approved modes of culinary preparation, are still immeasurably inferior to the maternal breast-milk, which, in the vital warmth with which it is indued when imbibed from the nipple, possesses properties which the subtlest skill of the chemist cannot supply.

“It is notorious that foster nurses, both in this country and elsewhere, are chiefly supplied from among the unfortunate class of illicit mothers, whose offspring are necessarily consigned to the charge of indifferent persons, who, if not totally ignorant of infant management, are certainly devoid of many of the qualities essential to the well-being of the child, and which none but its mother possesses. Nor are these foster guardians at all times over-scrupulous as to the means they employ for the purpose of procuring repose in the event of restlessness; whether arising from disease, or from deficiency or improper quality of food. It is well known that cordials, in the form of patent soothing mostrums and spirituous liquors, are often nefariously administered for such purposes, and possibly contribute not a little to the augmentation of the death-rate among this class of infants.

“According to official returns for the year 1847, the comparative estimate of legitimate and illegitimate deaths at different stages of the first year of life, for the whole of France (except the department of the Seine), stands as follows:—

Age.	Deaths (first year) to 100 births in 1857.	
	Legitimate.	Illegitimate.
0 to 7 days.	2·61	5·02
8 „ 15 „	1·93	5·53
16 „ 22 „	1·15	2·80
23 „ 30 „	0·86	3·07
1 „ 3 months	3·20	6·91
3 „ 6 „	2·78	5·28
6 „ 12 „	4·10	5·85
	16·63	34·46

"Thus, the rate of mortality of illegitimate children during the first year is shown to be more than double that of those born in wedlock; and the comparative estimate for the first three, as compared with the remaining nine months, is still higher, as may be better understood from the following summary of the preceding table:—

Age.	Deaths (first year) to 100 births in 1857.	
	Legitimate.	Illegitimate.
0 to 3 months . . .	9 75	23 33
3 „ 12 „ . . .	6 88	11 13
	<hr/> 16 63	<hr/> 34 46

"It is impossible to adduce an argument in favour of maternal breast-nursing more potently convincing than that which the preceding figures supply.

"In the case of the illegitimate there is another cause whereby the rate of early mortality is materially augmented—the absence, namely, of timely medical aid in the event of sickness. This is not necessarily owing to unskilfulness on the part of the practitioner employed: it is more probably due to neglect or delay in summoning such aid when required—a resource, however, which must, no doubt, be often much influenced by scantiness of means."

ART. 10.—*On the Prevalence of Suicide in England.*

By Mr. J. N. RADCLIFFE.

(*Social Science Review*, October, 1862.)

The following are notes of a paper read before the Social Science Congress:—

During the five years 1852-56, according to the Registrar-General's returns, 5415 suicides were committed in this country (including Wales), showing an annual average of nearly six suicides (5·87) to 100,000 persons living at all ages, and of 26 to 10,000 deaths from all causes.

In 1838-40 the annual average of suicides amounted to a fraction more than 6 (6·2) in 100,000 population, and to 28 in 10,000 deaths from all causes. It would seem, then, that in the two periods, 1838-40 and 1852-56, the tendency to suicide was nearly stationary. There would appear, therefore, to be no sufficient reason for the very prevalent belief that suicide has of late years largely increased in the kingdom.

Again, the belief that England is "the classic land of suicide"—can no longer be entertained in the face of these figures. The number of suicides in France, during the seventeen years 1836-52, averaged about 8 (8·3) in 100,000 population—1 in 12,013 inhabitants. In England, as we have seen, the proportion in the two periods, 1838-40, 1852-56, was 1 in 17,039 and 1 in 16,129 inhabitants. But then the English statistics of suicide are at the best imperfect. The Registrar-General's returns do not show, probably by

one-tenth, the whole amount of suicides actually distinguished as such at the time of death.

The Home Office returns of suicide now, however, extend over five years, 1856-60, and show an annual average of 6·7 per 100,000 population, or 1 in 14,906 inhabitants; a proportion in excess of the Registrar-General's returns for 1852-56, still comparing most favourably with those for France.

The positive records we possess certainly show that, as far as our present information extends, England holds only a second or third-rate position in the suicide scale among civilized nations.

The justness of this conclusion will become still more apparent by a glance at the Prussian statistics of suicide. In 1834, according to Dr. Morel, the proportion of suicides in that kingdom was 1 in 9,941 inhabitants, and in 1843, 1 in 8,081. In the three years 1850-52 the number averaged 38 in 100,000 deaths from all causes. Even France, then, as well as England, must yield the sad precedence to Prussia in this matter, unless the progress of suicide in France since 1852 has been such as to overtake that which had previously been observed in Prussia.

It is very noteworthy that the most recent statistical return on the health of the army shows a proportion of suicides occurring among the troops on the Home Station more than *double that found in civil life!* The proportion of suicides occurring among 1,000,000 males of the military age (20-40) in civil life, may be estimated approximately, according to the Registrar-General's returns for 1852-56, at 124·6. But the proportion occurring in the troops on the Home Station in 1859 (20 in a strength of 71,715 men) shows a ratio of no less than 278·8 in 1,000,000! It is difficult to escape the conclusion, even if subsequent returns prove that the proportion of suicide in 1859 was exceptional, that the causes leading to so extraordinary an excess of suicide among the troops at home, and those which have given rise to the recent outbreak of murders by soldiers, have much in common. However this may be, there can be no question that the army returns of suicide confirm the necessity, made too apparent by the late murders, for a careful inquiry into such grievances as may exist among the troops.

The returns of suicide in the navy show also an excess over those occurring in civil life, but not so great as in the army. The average proportion of suicides annually occurring among the sailors on the Home Station, to 1,000,000 of the strength, was, in the three years 1856-58, 135·4.

(B) CONCERNING ACUTE DISEASES.

ART. 11.—*On Epidemics.*

By Dr. STOKES, Regius Professor of Medicine in the University of Dublin.

(*British Medical Journal*, January 10, 1863.)

In his introductory address at Meath Hospital, Dr. Stokes says:—
 "What is it that causes an epidemic? is one of the most difficult

of questions. Why, in the pressure of an epidemic, one man sickens while another escapes—why essential maladies observe laws of periodicity, we cannot tell; why locality affects their general characters and organic results, is yet unexplained; nor why, on the other hand, after travelling their tens of thousands of miles, destroying their victims under the most varied conditions of climate, soil, food, temperature, they yet preserve much, if not all, of the original character, remains an open question. Many discussions have arisen on this point, and a fierce controversy has raged between the contagionists and non-contagionists. The advocates on both sides can bring forward plenty of isolated facts in support of their theories; but the argument from isolated cases is of little value. That there is an origin for epidemic disease, besides contagion, is self-evident; that it is propagated by contact, is another question. I have been in the habit of referring to the mode of settling this matter. It was to compute against the occurrence of a certain circumstance—namely, if there was no such thing as contagion. My father, when Professor of the Practice of Medicine in the Royal College of Surgeons, directed his attention very much to the subject of contagion. He was a strong advocate of the doctrine of contagion. Perhaps he went too far in his belief in the exclusiveness of the doctrine of contagion. He thought that in looking at the general circumstances which attended the spread of an epidemic in this country, the probabilities for or against the doctrine of contagion might be submitted to calculation. One of his most intimate friends was the late celebrated Dr. Brinkley, Bishop of Cloyne, who was at one time the Astronomer Royal of Ireland. He was admittedly one of the very first mathematicians of his day, and was especially skilled in that difficult part of mathematical investigation, the doctrine of chances. Now, in the progress of an epidemic in Ireland, in a family of twelve persons, the disease has been known to attack eleven out of the twelve. In some cases the passing of the fever through so large a proportion as eleven individuals out of twelve has taken a very considerable period of time, as you may readily understand. It has taken about three months to go through them all. Now, my father proposed these two problems to the Bishop of Cloyne for solution:—‘An epidemic prevails so severely that one person out of seven sinks. A family of twelve is selected in a particular district before the epidemic has visited it. What is the chance that eleven out of the family shall take the disease, supposing the sickness of one of the family does not promote the sickening of another—that is, supposing the disease not to be contagious, and supposing the family to be not unusually liable to the disease?’ The answer furnished by Dr. Brinkley is, that the probability against such an event is 189,600,000 to 1. That is a very singular and extraordinary result. The whole subject is yet to be worked out. It is interesting to observe that Humboldt, in his *Cosmos*, speaking of epidemic diseases, says that their origin and nature is among the most difficult of problems, and he suggests that some light may be thrown on the matter, when the laws of terrestrial magnetism are fully determined. What we do know of these mysterious phenomena may be thus

stated:—That, although having certain characteristics which are common to all, they exhibit marked and special differences. Thus the plague differs from cholera, cholera from typhus, and so on. That they are in a greater or less degree propagated by contagion. That epidemics of the same disease have not always the same characters. That their mortality is greater on their first outbreak in any locality. That they travel over vast distances, and although they may arise in warm latitudes, they preserve their characters in cold ones; the cholera of Central India and of St. Petersburg was the same disease. That their advent and disappearance are often sudden. That they are not symptomatic of any known anatomical change, but they often induce local diseases which are secondary to the general malady. That their symptoms are, to a great degree, under the laws of periodicity. That, as far as we know, the disease is not to be met by any specific cure. That, as yet, all explanations of their origin are insufficient or obviously erroneous. Lastly, that anatomy only throws a negative light upon their nature, telling us rather what they are not than what they are. The plague of the Levant, which has preserved its character since the time of Thucydides; the black death; the sweating sickness; the typhus and yellow fevers, and the Asiatic cholera—in a word, the great causes of the wholesale destruction of man, act by some influence not yet discovered even by microscopic anatomy; so that we come to the strange conclusion that the diseases most fatal to man are those least connected with organic change, which, when it is met with, is secondary, inconstant, and insufficient to explain their symptoms. Such is the present state of our knowledge.”

ART. 12.—*State of Epidemic Diseases in Great Britain in 1861-62.*

By MR. J. N. RADCLIFFE, one of the Honorary Secretaries to the Epidemiological Society.

(*Proceedings of the Epidemiological Society*, December 1, 1862.)

This report refers to the twelve months ending September 30, 1862. The following is a recapitulation of some of the principal facts recorded:—The health *status* of the English population, as estimated from the unusually low rate of mortality throughout the year, was generally good, notwithstanding dearness of provisions and an excessive amount of pauperism. The health *status* of the Scottish population was markedly below the average, as shown by the large amount of sickness prevalent in the last quarter of 1861, and the high rate of mortality since the commencement of 1862. The average death-rate of Scotland, it is well to remark, is below that of England. Thus, during the six years 1855-60, the annual proportion of deaths in England was 219 per 10,000 population: during the same period in Scotland the proportion was 208. The high range of temperature in the winter months, and low range during the summer, in England exercised a favourable influence over the health, notwithstanding

much wet and variability of weather. In Scotland similar conditions of temperature and weather existed, but to an exaggerated extent; and the great changes which were experienced, and especially the undue humidity of the atmosphere, were apparently the fostering causes of the influenza and throat affections, which appear to have been more common there than in England. The epidemic diseases most prevalent in England were continued fever, scarlatina, measles, diphtheria, whooping-cough, and small-pox. In Scotland the same diseases, with the exception of small-pox; also, and more particularly, sore-throat, often assuming a diphtheritic character, and accompanied by diphtheria, played the chief part in the epidemiology of the twelve months. In both parts of the kingdom continued fever prevailed most commonly in the autumn quarter of 1861; and in England the affection would appear to have been more general in the northern than in the southern portion of the kingdom. In both countries scarlatina was widely prevalent in the northern districts in the last quarter of 1861; but the disease became more active in the southern district in the third quarter of 1862. Measles prevailed extensively, and in some instances very fatally, in the winter quarter in England. In Scotland the disease appears to have been most prevalent in the spring and summer quarters. Diphtheria was, in England, principally fatal in the autumn quarter, but the disease prevailed, more or less, in every registration district during the year. In Scotland the disease, together with sore-throat, appears to have been epidemic throughout the year. Whooping-cough was widely prevalent in England during the winter quarter; in Scotland, during the winter and spring quarters. Influenza was epidemic in Scotland in the autumn and winter quarters. Finally, small-pox broke out in many districts of England, but more particularly in the eastern, south-western, and northern counties, and in Yorkshire. In no instance did an outbreak of any of the different diseases referred to as occurring in England assume what may be termed "general proportions." The outbreaks were essentially local; but the dispersion of the various maladies, or of their centres of manifestation, over the kingdom,—the cropping out of exaggeration of these diseases in different localities, and the effects they apparently exercised upon the sickness and mortality of certain districts, without heightening the death-rate of the whole kingdom,—present a study of great interest. From this study it is reasonable to conclude that, in dealing with these local outbreaks of epidemic disease in ordinary periods, the best chance is afforded of warding off the widespread and more deadly outbreaks of extraordinary periods. The detailed mortuary returns for Scotland extend, as yet, only to the year 1857; but the returns for England are brought down to 1860. From the latter, then, may be obtained information as to the *status* of the several epidemic diseases most prevalent in the twelve months discussed, immediately prior to that period. Since 1857 the mortality from continued fever has slowly declined; in that year the deaths from this cause amounted to 19,916; in 1860 they were 13,012. In 1855 the mortality from scarlatina was 17,314; in 1856 and 1857, the number of deaths from this disease fell considerably,

the mortality in the latter year being 12,646. The year following the mortality increased enormously, becoming wellnigh doubled, the number of deaths from the malady being 23,711. In 1859 the number fell to 19,310; and in 1860 it became as low as 9305. Prior to 1855 deaths from scarlatina, cynanche maligna, and diphtheria were not separated in the Registrar-General's report. Whether the detailed reports of the Registrar-General will show an increase of the mortality in the whole of England from scarlatina, during 1861 and 1862, as great as occurred in London, cannot be predicted. It is certain, however, that the activity of scarlet fever was great in several parts of the kingdom. The deaths from cynanche maligna in 1855 amounted to 199; in 1858, to 1770; in 1860 the mortality from the disease had decreased to 376. The mortality from measles was largely augmented in 1858, and there was a steady increase in the number of deaths from the disease in the two subsequent years. The deaths registered from diphtheria in 1855 numbered 186; in 1859, 9587; in 1860, the mortality from this disease had decreased to 5212. The mortality from whooping-cough, in 1860, was the lowest since 1852; and the mortality from small-pox had declined from 6460 in 1858 to 2749 in 1860. The reduced rate of mortality throughout England which occurred in 1860 was chiefly due, Dr. Farr states, to the decline of the number of deaths from scarlatina, diphtheria, and diarrhoea. A decrease also in the mortality from small-pox, erysipelas, and cholera contributed to the favourable results. The most noteworthy fact in the epidemiology of the metropolis during the twelve months was the remarkable outbreak of typhus. In 1858, 1859, and 1860, typhus had become so rare a disease in London, that the question of converting the Fever Hospital to other uses was seriously entertained. In 1861 typhus again became epidemic; and since January, 1862, the number of admissions to the Fever Hospital for typhus has exceeded that at any period of the history of the hospital. Dr. Murchison attributes this epidemic to the artificial scarcity produced by the system of strikes, which had for some time previously disorganized the labour market, and the condensation of population caused by the arrival of labourers in the metropolis from the country in search of work.* The mortality from continued fever in London, doubtless owing to the outbreaks of typhus, was in excess of that of any like period since 1848. Indeed, the total mortality of the winter, spring, and summer quarters of 1862 (2839) from this disease was alone in excess of that of any year since 1848. The true source of this excess of mortality would not have been rightly understood except by the careful nosological and etiological distinction of the forms of continued fever insisted upon by Dr. Murchison. The mortality from scarlatina was but a little below that from continued fever during the twelve months,—the total mortality from the former malady being 3437; from the latter, 3463. Next in order of mortality was whooping-cough. Continued fever, scarlatina, and whooping-cough were the chief epidemic

* See Dr. Murchison's recently published treatise on the *Continued Fevers of Great Britain* (p. 52), for an account of this epidemic.

affections of the period under observation in the metropolis. The mortality from continued fever increased to a maximum in the second quarter of 1862. The mortality from scarlatina was at its maximum in the last quarter of 1861, then decreased throughout the two succeeding quarters, but largely increased again in the summer quarter of 1862. The mortality from whooping-cough was greatest in the first quarter of 1862. Diphtheria was still largely fatal, having occasioned 625 deaths. Mr. Radcliffe detailed the history of the outbreak of typhus at Preston, in Lancashire, to the date of reading the report: he dwelt also at some length on epizootic diseases, brought together the chief accessible information on epiphytics, and terminated his report by a brief analysis of the principal contributions to epidemiological literature in Great Britain during 1861-62. The chief diseases prevalent among domesticated animals were epizootic pleuro-pneumonia, and the vesicular disease of the mouth and feet. Scores of sheep suffered and were lost from *filario* in the bronchial tubes and abomasum; there were several local but very fatal outbreaks of influenza among horses; and an outbreak of *variola ovina* occurred in Wiltshire. The history of the latter outbreak, which occasioned great alarm at the time, will be imperfect until the official reports are published. There was no special disease among plants during the year.

ART. 13.—*The Epidemics of Yellow Fever in Bermuda.*

By Deputy-Inspector Dr. SMART, R.N.

(*Proceedings of the Epidemiological Society*, March 2, 1863.)

Without going further back than the close of last century, it appears that the dates of the successive outbreaks of fever there have been 1796, 1812, 1818, and 1819, 1837, 1843, 1853, and 1856. The following are the principal conclusions drawn by Dr. Smart from his extended researches:—1. That in the best recorded yellow fever epidemics of Bermuda there has been generally a coincidence of the same disease on the American coasts. 2. That on such occasions there has been an epidemic constitution, manifested by the prevalence of catarrhal affections in the spring, and of gastric affections in the early summer—these yielding to fever, which, at its climax in the autumn, assumed the type of yellow fever with black vomit in a greater or less proportion of the attacks. 3. That during these epidemic seasons comparative immunity has usually favoured the native population, and those of the European residents dwelling under good sanitary conditions. 4. That the most intense manifestations of the disease have arisen in crowded barracks and convict-hulks, especially when the healthy and sick have been kept together. 5. That inasmuch as in the worst instances recorded it has been found that removal from infected localities has been always followed by an almost complete exemption of those not already infected, by amelioration of the state of the attacked, and lastly by an early extinction of the epidemic character of the fever, it is

therefore just to consider the essential causes of the disease to operate, under ordinary circumstances, by material local agencies, rather than by those of person. 6. That sanitary measures are the means to be relied on upon the approach of the epidemic constitution in any locality. 7. That, in the event of an epidemic outbreak, the same measures are highly valuable, but the only measure of certain value then is removal from the locality, and, in the case of crowded communities, as in barracks, ships, &c., immediate dispersion into wider space of all persons within the range of the noxious local agencies. 8. That, although the direct proof of personal contagion be still wanting, there are ample reasons for concluding that the highest degree of local infection has been generated in the hospitals, naval, military, and convict, so that the malady has been propagated among the attendants as well as among the sick. 9. That with regard to hospital arrangements for the treatment of yellow fever, owing to the peculiar predisposition arising from the debility of ill health, yellow fever hospitals should be distinct and special, and under sanitary cordon; and that, considering the disadvantages of the climate of Bermuda, the minimum of space, even when perfect ventilation can be maintained, should not be less than 1500 cubic feet per man in fever wards. After alluding to the terrible mortality from this fever in several ships of the West India squadron in 1861, during the passage to Halifax, and after their arrival there, Dr. Smart remarks: "I must confess that such results, placed side by side with those of immediate removal of men from infected localities, as exhibited in Bermuda experience, have raised a doubt in my mind whether equal losses of life would have been incurred by the immediate removal of the crews from their ships into some suitable quarantine establishment in the West Indies."

ART. 14.—*On Recent Typhus in Lancashire.*

By Dr. BUCHANAN.

(*Lancet*, February 14, 1863.)

Since the great typhus-epidemic in Lancashire at the time of the Irish famine in 1847-48, there has been scarcely any of this disease in the cotton towns. In 1862, however, positive maculated typhus has made its appearance. The disease has been most prevalent at Preston, and next at Manchester. Several cases have been met with in Chorley, a town not far from Preston, and more recently at Accrington; and scattered attacks, still of true typhus, appear to have been observed at Salford and Blackburn.

As the chief interest centres in Manchester and Preston, Dr. Buchanan confines his remarks to the outbreaks in these towns. He visited Preston late in October, under the directions of the Privy Council. The earliest case of distinct typhus he could trace had occurred on July 7th, in a four-roomed cottage, 17, Castle-street, at some distance from the centre of the town. It was not known that the first patient had been exposed to contagion. In this cottage

eight persons had crowded by night into a room whose utmost cubic capacity was 800 feet. They were dirty, under-fed, and the boy who first felt ill had also been much exposed to the weather. The boy was removed to the hospital, and returned home on his convalescence. In the last week of August six other persons were attacked in this house. They were removed and the house was closed. Meanwhile, new cases of typhus had occurred in another part of the town, apparently without communication with the former. These were in a district that was afterwards subjected to the chief violence of the epidemic. The locality consisted of very confined and dirty courts, lying low, near the canal, and densely inhabited.

At the end of August a third neighbourhood, distant from either of the other two, became affected with typhus. A fourth outbreak, apparently unconnected with the rest, was seen in another central part of the town in the middle of September. By the end of this month cases had occurred in five out of six wards into which the town is divided.

The general progress of the outbreak in the town may be estimated from the following return of reported cases of typhus:—Cases occurring in July, 2; August, 8; September, 23; October (five weeks), 109; November, 89; December, 38; week ending January 3rd, 13—a third more than in the previous week, and double the number occurring in the week ending December 20th; week ending January 10th, 15; and since then a still further increase, though not to any great extent.

At the time of Dr. Buchanan's visit, at the end of October, the House of Recovery (fever hospital), which was constructed for forty patients, contained fifty-two cases of typhus. Afterwards there were upwards of sixty inmates at one time. In some wards the space for each bed fell short of 600 cubic feet, and the air was here very foul.

At the beginning of November a wooden building was erected in contiguity to the House, capable of containing sixty patients, with a space of 1500 cubic feet to each. This building was put up in ten days; but there was a lamentable delay before the patients were removed into it at the end of December. In the meanwhile, upwards of twenty persons in attendance on the sick had caught the fever, the medical officer and the master of the House of Recovery being among the number.

It deserves mention, for the sake of those who refuse to acknowledge an epidemic influence that does not show itself on the general death-rate, that in the September quarter of 1862 the mortality from all causes in Preston was very markedly below the average, 464 deaths only being registered, against 607 in the corresponding quarter of 1861. This subsidence was entirely among children under six years, and resulted, there is reason to believe, from the greater care bestowed by mothers on their infants during the time of industrial depression.

The following returns from the parish fever hospital will indicate the progress of the outbreak of typhus in Manchester. In July, 7 cases of the disease were admitted; in August, 8; September, 12;

October, 20; November, 25; December, 17. From December 29th to January 13th, not one case; but on the last-named day, two cases, and since then a few who had contracted the fever in adjoining wards of the workhouse. Scattered cases of typhus have probably occurred in Manchester from time to time, but very rarely since 1847-48. The earliest instance of true typhus in the Infirmary occurred at the end of May, and two cases were admitted in June. At present there is very little typhus in Manchester; but it would be premature to reckon on the complete subsidence of the disease.

Dr. Buchanan refers to his recent report to the Privy Council, "On the Health of the Operatives in the Cotton Towns of Lancashire affected by the Prevailing Distress," for a detailed account of the circumstances contributing to the outbreaks of typhus in Preston and Manchester and other towns. He is at a loss to explain why the typhus influence should have fallen chiefly on Preston. Manchester was exposed to the danger of imported typhus. The limitation of the fever to these towns chiefly was to be ascribed to the strict removal of cases to hospital; the maintenance of a high standard of relief, increasing almost every month; the liberal distribution of bedding and clothing as the winter has advanced; and the almost unprecedented mildness of the weather since November. November, which was the coldest month since the distress began, witnessed the maximum of typhus cases both in Preston and Manchester.

ART. 15.—*Further Observations on Typhus and Typhoid Fevers, as seen in Dublin, especially the united form they assumed during the first half of the year 1862.*

By Dr. HENRY KENNEDY, Physician to Sir Patrick Dun's Hospital, Dublin.

(*Dublin Quarterly Journal of Medical Science*, August, 1862.)

About two years ago Dr. Kennedy endeavoured to show that typhus and typhoid fever were the result of a common poison: on the present occasion he publishes the results of his additional experience, giving the details of forty cases illustrative of the types of fever prevailing in Dublin during the last two years. These cases present examples "of typhus in both young and old;—of typhus without spots; of typhoid with none, with one or two, or with an extensive crop of them;—of typhus with the brain wonderfully free;—cases of typhoid, but more numerous, the same;—of both typhus and typhoid in which the state of the tongue and parts about were identical from sordes;—many cases of either type with the chest not engaged at all, or so slightly as not to call for treatment;—instances of both types with and without tympany;—cases of either kind entirely free from hæmorrhage; a freedom remarkable when compared with former years;—and lastly, the modified types of fever which the present year has disclosed."

"The conclusion," Dr. K. says, "at which, after the fullest consideration of this question, I have arrived is the same as that of two years since; but with still stronger convictions on the point. *I believe that the two fevers known as typhus and typhoid are the result of a single poison; and that no other hypothesis can explain so well all the difficulties of the case.* I consider, further, that those who hold for a plurality of poisons, are bound to explain the facts already given in this paper. They should tell us why the symptoms of those two affections so often run the one into the other; why the same type of fever, whether typhus or typhoid, presents such marked contrasts; why typhoid may assume the characters of putrid, ataxic, or inflammatory fevers; febricula, meningitis, &c., and still be typhoid all the time; and this, be it observed, is described by those who believe in the two distinct poisons. They will also have to answer the argument taken from analogy, and tell us, if scarlatina affords the most marked contrasts, why fever should not do the same; also, how it has happened that symptoms which one writer considers essential to the natural history of typhoid, are ignored or made little of by another. And, in the last place, an explanation must be given of what has occurred in Dublin this year—that is, the union of typhus and typhoid in the same subject. Now one and all these points may be satisfactorily explained on the idea of the existence of but one poison. I confess, however, it appears to me impossible to explain them on the theory of two. But, if we admit two, why not more? for assuredly there are other types of fever just as distinct as typhus is from typhoid. And this leads me to notice the third type, which prevailed in Dublin, and particularly among the middle ranks; I mean gastric fever. I hold that it is essential it should be distinguished from typhoid fever, with which it has the nearest connexion, were it for no other reason than that its treatment is very different. Several cases of it have been already detailed; but, except to notice it as a special type of fever, I am not about to speak of it further here. It was, I believe, to this type of fever that the cases given, which might fairly be called anomalous, are chiefly to be referred."

ART. 16.—*On the Treatment of Typhoid Fever.*

By DR. HENRY KENNEDY, Physician to Sir Patrick Dun's Hospital, Dublin.

(*Dublin Quarterly Journal of Medical Science*, August, 1862.)

"We know," says Dr. Kennedy, "that of late years anything of what would be called active treatment has been most materially modified, if not quite given up. Thus in Bartlett's work, which appeared in 1847, the measures recommended included the regular antiphlogistic treatment; whilst five years later Flint speaks of much milder measures being adopted. The late Dr. Todd, of London, we know, strenuously recommended the stimulant plan; which, it must

be allowed, he carried as far as any discretion would justify; and in a published lecture of the present year, Dr. Warde, of the Dreadnought Hospital, London, has advocated the leaving the disease very much to itself. Now I mention these plans, not to criticise them, but to state that each, in its turn, will be found useful, and that no physician who has fever to treat on the large scale will bind himself to one or the other. Every single instance must be treated by itself, and symptoms must be met as they rise. If this be done, I believe the typhoid type to be the most amenable of the many forms of fevers, provided it be seen in an early stage of the disease. Speaking of it as I have generally seen it, I would say it is not a fever to be left to itself; and several of the cases which have been detailed prove this; for there was no amendment till treatment was put in force: on the contrary, some of them were going from bad to worse. When, then, the case calls for it, and this is to be learned from the local as well as the general symptoms, I never hesitate to have leeches applied over the right iliac region, followed by a poultice; taking care the bites do not bleed too long. A more common plan, however, is the application of a blister to the same part: nor can I doubt the great value of such means, and believe it is not as generally used as it might be. The blister may be repeated with the best results; nor should we ever forget that the local disease with which we have to contend is very apt to be slow in yielding; that a relapse may readily occur, and when this happens the disease is rendered very much more grave than it was. Our object, in truth, is to prevent ulceration; for if this once occur the chances of recovery are materially lessened. I state this because a recent writer speaks of the disease as if ulceration must necessarily take place. I believe this is an erroneous way of considering the matter; and that we can, by treatment, anticipate, and so prevent it. '*Obsta principiis*' is all-important here, and a principle never to be forgotten.

"Of the internal treatment I have had no occasion to change from what was spoken of in the former paper. As an astringent I find the dilute sulphuric acid, in the proportion of one to three drachms to the eight-ounce mixture, by much the best remedy. No other of the class of astringents seems to me to act at all so satisfactorily; and it can be modified, with the greatest nicety, to the demands of each particular case. I have often seen medicines, such as chalk, gallic acid, lead and opium, unavailingly used; and then from the moment this acid was given the patient began to amend. But it is not to be used without discretion; for it may check the diarrhœa too suddenly, and the chest or brain may so become engaged: hence, it is best to begin with a moderate dose, and increase if the necessity arise. The rule is that the diarrhœa is to be gradually lessened; not suddenly stopped. In mild cases I find the acid infusion of roses a very suitable medicine; and, when there is pain, from two to six drops of laudanum, in each dose of the mixture, commonly answer well. The sulphuric acid, I need scarcely add, is the favourite remedy with Huss. When there are signs of irritation in the colon, and more especially when there is tenesmus, an anodyne enema acts like a charm.

"Dr. Warde, to whom I have before alluded, speaks of salines as being suited to the treatment of fever of the typhoid type. Such may answer in London; but with us in Dublin they would be positively injurious. Their effects on the healthy frame are quite too powerful to suppose that they would not act equally so on the frame weakened by a disease like fever; of which the best treatment now avowedly is, what may be called conservative. To the class of salines I would add the carbonate of ammonia, which I believe to be too indiscriminately used; and which, in my own experience, does not suit the type of fever of which I am speaking. I have known a very few doses of it bring on diarrhoea, not only in this fever, but in many other diseases; and, if my memory serve me right, I have seen a similar remark made by Sir Benjamin Brodie; and would hence hold out a warning against the use of either salines or alkalies in all diseases of a lowering type.

"There is a class of cases of the typhoid fever in which, without any interference, the diarrhoea suddenly ceases; whilst the chest, or it may be the brain, gets as suddenly involved. All such I have found turn out most critical, and I have latterly been in the habit of keeping up, for some days, a discharge from a small blister, usually put on the chest. In this way I think I have seen very beneficial results follow. It seemed as if the poison were, in part at least, got rid of by the system; and all went smoothly afterwards. The point, I believe, is worth bearing in mind, and so is mentioned.

"In the last place I would notice a point which was also spoken of on a former occasion. Are stimulants, as a class, used too indiscriminately? I think they are. It seems a very general impression that if they are to be used, it matters little of what kind they are: hence, brandy, wine, and beef-tea are constantly spoken of as being given to the same patient. Now I do not deny that all may be required at the same time. But I do say that in numerous instances judgment is to be exercised; for most assuredly the effects are not the same; and when their different composition is considered, this need not excite wonder. Thus, if we compare wine and beef-tea, the former, contrary to what might at first be thought, may be given with much less risk than the latter; and I am sure I have seen cases where secondary inflammations—in the chest amongst other parts—have been lighted up by want of attention to the very point of which I speak. Though much more might be said on this subject, enough has been advanced for my present purpose.

"In conclusion, I would observe that the class of mixed cases, as they may be well called, require even more than the ordinary amount of attention. The fever becomes so heavy in many of them that the abdominal symptoms are very apt to be masked, and so may readily be overlooked. In such cases, too, it may be requisite to direct our treatment at one time to the chest, or again, to the brain; and, in some of the cases given, a combined treatment had to be adopted."

ART. 17.—*On the Inhalation of Nitrous Oxide Gas in severe Cases of Fever.*

By Dr. SHUWARD, U.S.V. Medical Director, Danville District, Kentucky.

(*American Medical Times*, January 17 and 24, 1863.)

After mentioning certain reasons which suggested the practice, Dr. Shuward says:—

“Without attempting an analysis of the cases related in this paper, I will merely remark that all the patients to whom the remedy was administered were pronounced hopeless by their attending physicians, and that their judgment in the matter was fully confirmed by that of the committee appointed to examine the cases before the gas was inhaled; that a striking improvement was observed in every case after the gas was administered; that under its influence warmth slowly returned to the extremities, after the most powerful diffusible stimulants that could be given had failed to produce that result; that the pulse increased in volume, and became much more natural to the touch; that the delirium which had in several of the cases existed for weeks previously entirely subsided; that the involuntary discharges from the bowels in all but one case ceased; that several of the cases, after lying for many hours delirious or insensible, became rational and conversed with those around them; that the countenance assumed a much more natural expression; that the livid spots upon the chest and abdomen of two of the cases changed to a light rose colour, and commenced disappearing; that the patients all expressed themselves as feeling much better; that the effects of the gas were not merely temporary, but permanent; that in the four cases that have died, life was apparently prolonged many hours by the remedy; and that three out of the seven supposed fatal cases are still living, and may yet recover.

“I propose to continue the experiments, and shall hereafter not confine them alone to cases that are considered hopeless.

“Although it has thus far been tried in only eight cases, the results are sufficient to prove that we have in oxygen gas a remedy of surprising power, and one that bids fair to be of great service hereafter in the treatment of almost every variety of disease.

“The gas was administered to all the cases in the form of nitrous oxide, which was made in the usual manner from nitrate of ammonia, by Prof. Brikford of Danville, Kentucky, and Assis. Surgeon Semmler, U.S.V. For want of better apparatus, it was administered to the patients from beef bladders, which answered the purpose moderately well.

“Although oxygen was employed in these cases in the form of nitrous oxide gas, I would not propose to use it so in all cases. In cholera, and severe cases of congestive chill, I am persuaded that oxygen gas, in its pure form, or slightly diluted with atmospheric air, would be better; nor would I hesitate to give it in any form of

disease in which the vital powers are depressed, since the cases recorded show that it relieves delirium and irritation, instead of producing them."

Of the eight cases related we copy two :—

CASE I.—J. B., age about 35 years. Has been labouring under typhoid fever, with its usual symptoms, for about three weeks. When examined on the 22nd inst. his condition was as follows :—

Patient lying upon his back ; jaw depressed, eyes sunken, pupils elevated, breathing laborious ; patient delirious ; extremities cold, clammy, and nearly insensible to the touch ; surface of chest and abdomen thickly marked with irregular-shaped, dark-coloured spots, varying in size from that of half a dime to that of a twenty-five cent-piece (petechiæ) ; pulse 64 per minute, small and hardly perceptible at the wrist ; involuntary discharges from the bowels ; teeth covered with dark sordes ; tongue dry, fissured, and dark coloured.

The case was pronounced hopeless, and the medical officers present were unanimous in the opinion that he could not survive longer than a few hours. Under these circumstances nitrous oxide gas was administered to him by inhalation at nine o'clock P.M. The following were the results :—

First minute, while breathing it, the pulse remained at 64 per minute, but a slight increase in volume was perceptible ; second minute reduced to 44 per minute, volume about the same ; third minute pulse 88, volume perceptibly increased ; fourth minute 90, volume about the same ; fifth minute 98, no change in volume ; sixth minute 92, seventh and eighth minutes 90, ninth minute 84 and fuller, tenth minute 90, volume about the same ; eleventh and twelfth minutes 88, full and strong ; thirteenth minute 92 ; fourteenth, fifteenth, and sixteenth minutes 88, volume about the same ; seventeenth minute 94 and full, eighteenth minute 96, nineteenth minute 98, twentieth and twenty-first minutes 96, fulness about the same. The gas was now discontinued.

At the end of the first half hour after its discontinuance the pulse was 80 per minute and full ; end of the second half hour, pulse the same ; end of third half hour, pulse the same, and extremities becoming warm ; end of fourth half hour, condition of patient about the same ; end of fifth half hour, pulse the same, extremities becoming warmer ; end of sixth half hour, pulse about the same ; end of seventh half hour, patient warm and rational, pulse about the same. Has taken some nourishment in the form of concentrated essence of beef ; says he feels better.

At the end of four hours the gas was again administered ; the pulse remained at about 80 per minute and full, while the extremities continued warm and the patient rational.

Seven hours after the second inhalation the pulse became weaker, and increased in frequency. The supply of gas being now exhausted, and not having the proper materials at hand for manufacturing it, we were prevented from giving it a third time, and the patient died thirty-two hours after the gas was first administered.

Nine hours after the gas was first inhaled by the patient the petechial spots upon the surface of his chest were observed to undergo a marked change in colour, becoming much lighter coloured, while some of them disappeared entirely. Several of the spots upon the surface of the abdomen also became much lighter coloured.

CASE IV.—A. B. W., age about 25 years. Has been suffering with typhoid fever, with its usual symptoms, for fourteen days. When examined on the 23rd of November he presented the following symptoms :—

Patient lying upon his back ; extremities cold ; surface of chest marked

with small rose-coloured spots; eyes sunken, pupils elevated; jaw depressed; breathing short and laborious; tongue dry, fissured, and dark brown colour; patient delirious; pulse 96 per minute, small and feeble.

The gas was administered to him at nine o'clock A.M. End of the first five minutes, while inhaling it, pulse reduced to 90 per minute; end of eight minutes, pulse the same in frequency, but stronger. Gas suspended. Patient examined one hour and a half afterwards: pulse 90 per minute, and fuller; extremities becoming warm; patient more rational, says he feels better, and desires more gas; breathing more regular and less laborious.

At five o'clock P.M., extremities warm; pulse 96 per minute and moderately full; patient rational. Gas administered. End of ten minutes, pulse 96, volume good. Gas suspended. Soon afterwards the patient took some nourishment in the form of concentrated essence of beef.

For the want of material for manufacturing it, the gas was not again administered to the patient until November 26. At that time he presented the following symptoms:—

Pulse 100 per minute, and feeble; breathing somewhat laborious; patient rational; extremities warm. Gas administered at eight o'clock A.M.; at the end of ten minutes, pulse 96 per minute, volume increased, breathing less laborious. Patient continued in very nearly the same condition until three o'clock P.M., when the gas was again administered. At the end of three minutes, while inhaling it, pulse 100 per minute, volume good; eighth minute, pulse 104; thirteenth minute, pulse 108. Gas discontinued.

Patient examined at a quarter past seven o'clock P.M.; pulse 124 per minute, and weaker; patient rational; extremities warm; rose-coloured spots on surface of chest disappearing.

Gas again administered. After inhaling it five minutes, pulse 105, volume increased; tenth minute, pulse 100 per minute, and moderately full and strong. Gas discontinued.

November 27, half-past eleven o'clock. Patient rational, and converses with those around him; says he feels better; extremities warm; has taken nourishment; pulse 100 per minute, full and moderately strong.

Gas administered. At the end of ten minutes, pulse 95 per minute, and full; end of fifteen minutes, pulse 100, volume the same. Gas discontinued. Patient examined thirty minutes afterwards; pulse and other symptoms about the same.

At six o'clock P.M., pulse 72 per minute, and full; patient warm and rational. Gas administered. In five minutes, pulse 84 per minute, and full. Gas discontinued.

At eight o'clock P.M., pulse 100, and full; at half-past nine P.M., pulse 110, volume good. Gas administered. Eighth minute, while inhaling it, pulse 106, volume about the same; fourteenth minute, 100 per minute. Gas suspended.

At half-past eleven o'clock P.M., pulse 94 per minute, and full; patient rational; extremities warm.

Patient examined at eight o'clock the following day, November 28. Pulse 100 per minute, patient rational, and converses cheerfully; has taken food several times during the night.

At five o'clock P.M., pulse 96, and moderately full and strong. Gas again administered. Fifth minute, whilst giving it, pulse 100 per minute, volume about the same; ninth minute, pulse 96; twentieth minute, pulse the same in frequency, volume increased. Gas discontinued.

Patient examined at half-past nine o'clock, November 29. Pulse 96 per minute, volume reduced. Gas administered. Fifth minute, whilst giving it, pulse 92, volume increased; twentieth minute, pulse 96, volume good.

Patient examined at five o'clock P.M. Pulse the same, tongue moist, extremities warm, skin slightly moist, patient feels cheerful.

Case still under treatment.

Ten o'clock P.M., December 5. Patient still under treatment; condition about the same.

ART. 18.—*On the Treatment of Acute Rheumatism by Nitrate of Potash.*

By Dr. BASHAM, Physician to the Westminster Hospital, &c.

(*Medical Times and Gazette*, December 20, 1862.)

Twelve years' additional experience, Dr. Basham tells us, has confirmed in every respect the principles enunciated in the paper in which he first proposed this mode of treatment. During this time every case of acute rheumatism passing through his wards has been treated by large doses of nitrate of potass, plentifully diluted, combined with the local application of the same salt to the joints; and the general average of the duration of the disease has been not more than three or four weeks. Dr. Basham says:—

"There is present in acute rheumatic fever a very striking and exceptionable state of the blood, marked by the predominance or great excess of the fibrin of the blood. The average proportion of fibrin in healthy blood is about two parts in a thousand, but in acute rheumatism it often exceeds six: nor is this all—the saline constituents play an important part in the composition of this fluid, and ordinarily may be estimated at about twelve parts in a thousand. But in acute rheumatism the proportion descends as low as eight, or even seven. Here are two important pathological facts,—the absolute excess of fibrin, and the absolute decrease of the salts. Now, although we are not certain that we can demonstrate the positive relation which the saline constituents bear to the fibrin, yet by analogy we can infer that some important connexion exists between them. Thus, the fibrin of the blood will not separate or coagulate, as it is called, if blood be received into a vessel containing certain neutral or even alkaline salts. But the most important fact is that observed by Dr. Stevens, in the state of the blood in a person who, by mistake, had swallowed an ounce of nitre in solution, mistaking it for Glauber's salts. In those days it signified little what symptom a patient exhibited, or what accident he might suffer; the first act was to open a vein. This man, who had swallowed this large dose of nitre, was bled, and to the astonishment of Dr. Stevens, the venous blood came from the vein almost as bright as arterial, and continued fluid, without exhibiting the usual characteristic of spontaneous coagulation.

"Now, let me apply this fact to the treatment of acute rheumatism by nitrate of potash. First, we know that in the middle of the last century large doses of nitrate of potash were successfully given in this disease. Secondly, we now know that nitrate of potash, circulating in the blood, prevents the separation or coagulation of the fibrin. And lastly, in acute rheumatism we know that there is not

only a large excess of fibrin in the blood, with a diminution of the salts, but we know also that there is a special tendency for this fibrin to escape from the circulatory fluid, and to form the exudative products which are thrown out in the inflamed tissues, and which constitutes such an element of danger when it is poured out on the pericardial or exocardial surfaces, or forms deposits on the aortic or mitral valves. It is a fair and rational inference, then, that a salt which can prevent the coagulation or separation of the fibrin when present in the circulatory fluid in a person in comparative health, will operate in a similar manner where the blood is contaminated by excess of fibrin.

"The chances of inflammatory fibrinous products being poured out are thus lessened, and time is obtained for the natural processes to reduce the excess of fibrin and to restore the blood once more to the standard of health. Such, then, is the reasoning applied to the employment of nitrate of potash in acute rheumatism. But recollect that these doctrines are applicable alone to that form of rheumatism which is usually denominated acute, and which is always accompanied by manifest local conditions of inflammation about the joints—principally of the hands, elbows, shoulders, ankles, and knees. This treatment is not adapted to those forms of rheumatism which are called either chronic, or muscular, or fibrous, and characterized chiefly by wandering pains in the limbs or various parts of the body, and which are never accompanied by any disposition to local redness or tumefaction. If the principle be steadily kept in view on which the nitrate is employed, there is little chance of the error of employing it in those forms of rheumatism which are not dependent on, or connected with, a hyperinotic state of the blood. I cannot forbear noticing here an objection which has been raised by Lehmann, to what he conceives to be a pharmacological error on the part of those physicians who believe in the antiphlogistic power of the nitrate of potash. He says it is difficult to draw the conclusion that nitre can prevent the formation or augmentation of fibrin in inflammatory blood, simply because coagulated fibrin is soluble in a solution of that salt. In such a conclusion I desire to express my perfect concurrence. I have never adopted such reasoning: I have always believed that the nitrate of potash does not lessen the development or formation of fibrin in the blood, or in any way regulate the quantity formed. The opinion which I have ventured to express, and the explanation I have put forward to interpret the action of this salt in inflammation, are based on the following facts:—

"1. Nitrate of potash, when present in the blood of a living animal, retards or suspends the separation of the fibrin.

"2. In inflamed blood, there is a considerable increase in the proportion of fibrin, and the essence of the inflammatory process consists in the exudation or escape of this constituent of the liquor sanguinis, and its deposit in the tissues.

"3. The presence of nitre, even in small quantities, in the circulating fluid, suspends this disposition of the surplus fibrin to form exudations; while other agents, or even natural causes, may be

operating to reduce the proportion of fibrin and bring it within healthy limits.

"The first is a physiological fact, proved in its direct applicability to man by Dr. Stevens' case of the non-coagulability of the blood of one who had swallowed a large quantity of nitre by mistake. The second rests partly on chemical, partly on pathological proof, and cannot be disputed. The third rests purely and solely on clinical observations: it is to a certain extent hypothetical, and incapable of direct proof—but it is based on the constant sequence of marked and striking exemptions from the usual consequences of the inflammatory process, and which are not observed to follow with so much certainty or regularity when other and different means are employed. Lehmann further states an objection to the assumed properties of nitrate of potash, founded on the quantity of nitre usually administered in the twenty-four hours as being inadequate to the solution of the fibrin present in the blood. Assuming the quantity of blood to amount to twenty pounds, and that the fibrin amounts to three parts in a thousand, then the quantity of fibrin would be somewhere about 300 grains; and he contends that 200 grains of nitrate of potash would be necessary to dissolve this quantity, whereas not more than ten grains are administered every two hours, and the whole quantity in twenty-four hours would not exceed 100 or 120 grains. And as the salt passes off so rapidly by the kidneys, it must be prevented from accumulating in the blood. But, in answer, it may be stated that patients constantly take 480 grains in the twenty-four hours when freely diluted. That the salt passes rapidly off by the kidneys is well known; but by frequent repetitions of the dose of the salt, a sufficient quantity is constantly kept present in the circulating fluid, not to dissolve all the fibrin, but to retard or prevent any excess forming inflammatory exudates when a state of hyperinosis exists. This author further states that it would be very singular and inexplicable why other salts which equally possess the power of dissolving fibrin should not be classed with nitre as special antiphlogistics; for instance, alkaline carbonates, which are remarkable for preventing the coagulation of the blood. It is not without hesitation that I venture to remind so distinguished a physiological chemist, that other salts are equally efficacious, and that the alkaline carbonates have been proved by clinical observations to possess this power of suspending or modifying the exudation of fibrin. But many of these salts have special physiological properties, some acting as purgatives, as the sulphate of soda; others as diuretics and purgatives, as the acetates. The alkaline carbonates have also been proved to exercise modifying properties; but the objection to their employment rests, as has already been shown, on their marked interference with the functions of the stomach when given in sufficient quantity to exercise an influence over inflammatory exudations. The nitrate of potash in these respects possesses certain negative properties which adapt it for administration in preference to other salts.

"When first I commenced the employment of the nitrate of potash in acute rheumatism, and indeed up to the period of the communi-

cation to the Medical and Chirurgical Society on this subject, the nitrate was used in co-operation with other remedies, such as calomel and opium, antimony, and even colchicum. But I have long since ceased to employ any of these latter remedies, experience having convinced me that the real efficient agents in this acute disease are opiates and salines. Brisk purgatives in some cases are desirable at the outset of the treatment; but subsequently they are not necessary.

* * * * *

"It is not a little singular that a remedy so efficient in rheumatism, so unequivocally remedial, tested by the experience of many hundred cases, which are uniformly treated here on one definite plan, should scarcely yet be recognised by the profession as specially applicable to the treatment of acute rheumatism. No doubt it takes a long time for a remedy to win its way into the confidence of the profession. It is not sufficient that the principles on which it is employed are sound and intelligible. It must yield in the hands of each practitioner similar and uniform results; and through the testimony of the many, it at last secures for itself a permanent confidence in its utility. It is a century since a most distinguished metropolitan physician asserted its pre-eminent efficiency in acute rheumatism; and there have not been wanting physicians in recent times to bear witness to the success of cases treated by this salt. In France its efficacy has been recognised by Gendrin, Martin Solon, Bonnet, and others, and yet it is far from being accepted in this country and employed as universally as its utility demands. And why is this? I think the explanation is not difficult. Sufficient attention has not been paid to the fact, which I have never failed to inculcate to all the pupils of this hospital, and have laboured most earnestly to engraft it on your minds, that it is only in the acute inflammatory rheumatism—the form of disease which is usually termed rheumatic fever—that this salt proves efficacious. Acute inflammatory symptoms in the joints, with febrile disturbance, are the indications for its use. But the term 'rheumatism' is, unfortunately, of very wide and inexact application. It comprises many forms of very different morbid processes. Wandering pains in the limbs are called rheumatism; muscular pains are called rheumatism; aching pains from whatever cause, the sequel of malaria, of syphilis, of mercury, all are recognised by this common term. Certain inflammatory conditions affecting the joints, more particularly of females—a true arthritis, sometimes called rheumatic arthritis—are examples of the inexactness with which one common term continues to be applied to very different morbid conditions. And here is found the explanation of the apparent failure of nitrate of potash in the treatment of rheumatism. It has been tried and used in cases in which it is totally inapplicable. I have frequently had the remark made to me by practitioners, that they have used the nitrate and found no advantage in its action. Upon inquiry it has always been proved that the cases were not acute rheumatic fever, but some form of rheumatism, the pathology of which differed materially from that of the acute disease now under consideration. I continue,

however, to receive the testimony of many able and distinguished physicians, who have carefully employed the nitrate in appropriate cases, that its efficiency is equal to what Dr. Brocklesby claimed for it, and that the results to which I called the attention of the profession in 1848 have been confirmed; and that cases thus treated are, to a great extent, exempted from cardiac complication; and that, if attacked with heart symptoms, the inflammatory process is mitigated and controlled; and also that the duration of the acute stage and the intensity of the inflammatory symptoms are diminished; and the convalescent period—which every one who recollects when acute rheumatism was treated by bleeding, calomel, antimony, and opium, knows to have been most tedious and protracted—at the present time is seen to be moderate and limited, the patients quickly recovering their vigour and bodily energy. One cause of this more favourable stage of convalescence arises from the diminished destruction of the blood corpuscles which occurs in cases of acute rheumatism treated by the nitrate of potash. That peculiar pallor and exsanguinous look which was so characteristic of the convalescent stage of patients treated by bleeding and antimony is rarely seen to the extent formerly witnessed. There is thus less reparative work to be done. Nutrition and stimulants in moderation more quickly restore the blood to a healthy and invigorating condition; and the healthier, even ruddy, aspect which quickly follows such a treatment is the best proof that these principles of therapeutics are both practically and theoretically sound.”

ART. 19.—*Remarks on the present Prevalence of Small-pox.*

By Dr. GEORGE BUDD.

(*British Medical Journal*, May 9, 1863.)

It is impossible to consider the wonderful protection against their own recurrence given by small-pox and other members of that peculiar group of diseases which, as a rule, occur but once in life, without having attention arrested by the *general* mitigation of small-pox that results when the infection, instead of being received through the air by casual exposure, is given by *inoculation*—in other words, by direct implantation of the infectious matter on the skin.

Before Jenner's memorable discovery of the power of cow-pox to protect from small-pox, when the practice of inoculation with small-pox matter prevailed in this country, it was estimated that, if the subjects for inoculation were well selected, small-pox communicated by inoculation was something like a hundred times less fatal than small-pox taken in the natural way.

Notwithstanding the remarkable mitigation of the disease by inoculation, the benefits resulting from the practice to society in the aggregate were not, as is well known, so great as might have been expected. The mortality among those actually inoculated was indeed diminished in about the proportion just stated; but many persons neglected to be inoculated, or to have their children inoculated;

many others entertained a prejudice against the practice ; and these were worse off than they would have been if the practice of inoculation had not been introduced, because this practice, by multiplying sources of infection, greatly increased their chance of taking the disease. It is seen, indeed, by reference to the bills of mortality, that, after the practice of inoculation had become general, the number of deaths from small-pox increased rather than diminished.

The effect of inoculation on the individuals inoculated, and on the race, which, through Jenner's discovery, had almost come to be a subject of mere historical curiosity, was again made of great practical interest by the outbreak in Dorsetshire, last summer, of the pustular disease in sheep, which from its striking resemblance to human small-pox has been termed *variola ovina*, or the small-pox of sheep. This disease resembles small-pox in the character of the pustules, in its severity, and in its communicability through the air and also by inoculation with matter taken from a pustule ; but there is reason to believe that it is specifically different from human small-pox. The disease is not prevented by a previous vaccination with the matter of cow-pox, as small-pox in man is : it is not communicated to the shepherd ; and all attempts to give it by inoculation to other domestic animals, even those most allied to the sheep, have failed. But, although apparently specifically different from human small-pox, it has been found, like human small-pox, to be very much less fatal when communicated by inoculation than when taken in the natural way ; and many of the flock-masters of Dorsetshire, at the risk of multiplying sources of infection, had recourse to inoculation to save their flocks.

The circumstance that inoculation mitigates this disease in sheep, as it does small-pox in man, warrants the inference that the mitigation by inoculation is not peculiar to this or that disease, but comes under some general law.

Now, when inoculation for small-pox is practised on a person susceptible of the disease, this is what happens :—The development of a small-pox pustule immediately begins at the inoculated spot. On the third or fourth day after inoculation, there may be seen at this spot, on an inflamed base, a small vesicle, which can already be recognised as a small-pox vesicle by the peculiar depression or cup-shape of the summit, that characterizes the vesicles of small-pox and cow-pox. Without much general illness, this vesicle becomes pustular and ripens, just like any individual vesicle of ordinary small-pox. On the evening of the seventh, or early on the eighth day, when the pustule at the inoculated spot is fully developed, fever occurs like the eruptive fever of natural small-pox, and issuing, as this does, on the second day, in an eruption of small-pox over the body. The disease runs its subsequent course exactly as natural small-pox does, except that, in the great majority of cases, it is much milder.

There is, then, in small-pox resulting from inoculation a twofold development of the small-pox pustule. First, in point of time, there is the development of a single pustule at the inoculated spot ; and when this pustule is ripe, or nearly ripe, a general eruption of small-

pox makes its appearance, to run its appointed course. In natural small-pox, on the contrary, small-pox pustules are developed once only. The virus has no visible effect for seven or eight days after infection; fever then sets in, issuing on the second day in a general eruption of small-pox.

We may conceive, then, that in inoculated small-pox there is a double infection: one simply local, resulting from the *engrafting* of the small-pox at the inoculated spot, which takes effect immediately; the other a general infection, which, as in natural small-pox, produces a crop of small-pox pustules after eight or nine days; and as small-pox, when it has once run its course, modifies the system in such a way that the disease cannot occur in that individual again, so the development of the single pustule at the inoculated spot has the effect of gradually exhausting, as it proceeds, the original susceptibility to the disease, and thus mitigates the effect of the general infection.

If this reasoning be just, the remarkable mitigation of small-pox effected by inoculation depends on the agency by which the disease prevents its own recurrence, and an important inference may be drawn. As cow-pox prevents future small-pox, at least for some years, as effectually as small-pox itself does, it may be inferred that, if vaccination be practised soon after the infection of small-pox is received through casual exposure, the development of the vaccine vesicle (which does not sensibly differ in rate of growth or appearance from the small-pox vesicle resulting from inoculation) will get the start of the small-pox infection, and thus have the same effect in mitigating the disease as inoculation with the matter of small-pox generally has.

When, therefore, small-pox occurs in a house, immediate vaccination may be recommended to all the inmates who can be supposed to be susceptible of small-pox infection, not only to prevent future infection from small-pox, but also with a reasonable expectation of mitigating the disease, should infection have already taken place.

By parity of reasoning, the mitigation known to be effected by inoculation in the small-pox, *so-called*, of sheep, would imply—what has not, that I know, been put to proof—that the disease in these animals cannot occur in the same individual twice.

ART. 20.—*Small-pox in London.*

By the Metropolitan Associate of Officers of Health.

(*British Medical Journal*, May 9, 1863.)

At a special general meeting of the Metropolitan Association of Officers of Health, held on the 25th April, at 8, Richmond-terrace, it was resolved unanimously to adopt the following circular, prepared by the General Purposes Committee, relating to the recent alarming progress of small-pox in the metropolis, and to the necessity of a combined effort to arrest the further diffusion of the disease:—"The Association having taken into their consideration the

present prevalence of small-pox in London, deem it to be their duty to call the attention of the guardians of the poor and other local authorities of the metropolis to the subject. They regret to find that the epidemic is still on the increase. In some of the metropolitan districts the mortality has already attained alarming proportions. It can scarcely be hoped that others equally populous will remain exempt, unless energetic measures are taken to arrest the further progress of the outbreak. While there can be no doubt that the prevalence of the disease is mainly attributable to the neglect of vaccination, and to the defective and unsatisfactory manner in which the operation is too frequently performed, and that if good vaccination were universal, small-pox would be almost unknown, it is not the less apparent that the prolonged residence of infected persons in rooms occupied by others, the exposure of such persons in the streets, in public conveyances, or in the waiting-rooms in hospitals, and the absence of adequate means of isolation, have been the immediate agents in bringing about the recent rapid diffusion of small-pox. During the present epidemic, these causes are in operation to a far greater extent than they were in that of 1859-60. In consequence of the insufficient size of the Small-pox Hospital, and the absence of any other provision for the reception of cases, persons having small-pox are daily applying in numbers for admission into the general hospitals. Every such application is necessarily refused; the sufferer is sent back to his home, there to become, against his will, a source of infection to his neighbours. The Association are of opinion that, whenever a case of small-pox occurs in a populous locality, the patient ought, in the interests of public health, to be removed as soon as possible; and that, in order to facilitate such removal to the utmost, it is not only necessary to provide for the gratuitous conveyance of small-pox cases, but for their reception and treatment. For this purpose, it appears to them indispensable that, during the continuance of the present epidemic, temporary buildings or wards should be opened in such situations as may be best suited for the purpose, on the double ground of distance from inhabited houses and facility of approach. As regards vaccination, it is desirable that every facility should be offered for the gratuitous vaccination of all who are willing to apply for it, whether they have been previously vaccinated or not; and that the times and places appointed by the guardians for vaccination should be notified by bills posted in prominent places. It is further necessary that, in all those districts in which small-pox is prevailing, and in which there is reason to believe that vaccination has been neglected, personal inquiries as to the cause of this neglect should be made by inspectors temporarily appointed for the purpose, who should be empowered by the guardians to take proceedings under the Vaccination Acts Amendments Act, 1861, in all cases of wilful refusal to comply with the requirements of the law."

ART. 21.—*On the Use of Quinine in Small-pox.*

By Dr. MOUSER, Physician to the Small-pox Hospital,
San Francisco.

(*Pacific Medical and Surgical Journal*, September, 1862.)

"In June, 1856," says Dr. Mouser, "I read an article on this subject before the Sacramento County Medical Society, founded on observations made in 1853. The article referred to was published in the first number of Dr. Morse's *California State Medical Journal*, July, 1856.

"At that time I was fully convinced of the efficacy of the remedy, and resolved to give it further trial should opportunity offer. It has not been my fortune to meet with an opportunity to test it since until March of the present year, when I took charge of the Small-pox Hospital in this city. My first observations having been made in a malarious district (Sacramento), left me in doubt as to the propriety of pursuing the same treatment here, where intermittents are not so prevalent, and other diseases seem to be much less modified by malarious influences.

"For the first two months I did not administer quinine to my patients, except where there seemed some special indication for it. I found twenty-nine patients in the house when I took charge of it, and admitted sixty more during the two succeeding months, making a total of eighty-nine treated during this time. Of these, forty-four were discharged cured, eighteen died, and twenty-seven remained.

"At this time (May 20th) I commenced the use of quinine, and administered it in two-grain doses every three hours, to all of my patients, from the time they entered the house until all febrile symptoms had subsided and desiccation was fully established. After which I ordered the same dose three times a day until they were entirely well. From May 20th to August 1st, twenty-five were admitted, making the whole number treated fifty-two; of whom forty-five were discharged cured, one died, and six remained.

"During the prevalence of this epidemic, I treated thirty cases outside the hospital, nearly all on the same plan, without the loss of a single one.

"Erysipelas prevailed to a considerable extent in the hospital, following closely the subsidence of small-pox, but was much less frequent after the adoption of the quinine treatment.

"How much of the difference in these results depends on medical treatment, and how much on circumstances other than this, I do not pretend to say. We should not lose sight of the fact that an epidemic on the decline is said to be less fatal than at its commencement. There is one other circumstance that probably had an important influence in reducing the mortality in the last two months, which is, the difference in the accommodations first and last. When I took charge of the place, eight patients were crowded into each room, about fourteen feet square; but some time in May ample provision was made for room, so that there was no more crowding."

ART. 22.—*Leopards and Small-pox in Ceylon.*

By Sir J. EMMERSON TENNENT.

(Dublin Medical Press, December 17, 1862.)

Leopards are strongly attracted by the peculiar odour which accompanies small-pox. The reluctance of the natives to submit themselves or their children to vaccination exposes the island to frightful visitations of this disease; and in the villages in the interior it is usual on such occasions to erect huts in the jungle to serve as temporary hospitals. Towards these the leopards are certain to be allured; and the medical officers are obliged to resort to increased precautions in consequence. This fact is connected with a curious native superstition. Amongst the avenging scourges sent direct from the gods, the Singhalese regard both the ravages of the leopard and the visitation of the small-pox. The latter they call, *par excellence*, "maha ledda," "the great sickness;" they look upon it as a special manifestation of devidosay, "the displeasure of the gods;" and the attraction of the cheetahs to the bed of the sufferer they attribute to the same indignant agency. A few years ago, the capua, or demon priest of a "dewale," at Oggalbodda, a village near Cattura, when suffering under small-pox, was devoured by a cheetah, and his fate was regarded by those of an opposite faith as a special judgment from heaven. Such is the awe inspired by this belief in connexion with the small-pox, that a person afflicted with it is always approached as one in immediate communication with the deity; his attendants address him as "my lord" and "your lordship," and exhaust on him the whole series of honourific epithets in which their language abounds for approaching persons of the most exalted rank. At evening and morning a lamp is lighted before him, and he is invoked with prayers to protect his family from the dire calamity which has befallen himself. And after his recovery, his former associates refrain from communication with him until a ceremony shall have been performed by the capua, called "awasara-pandema" or "the offering of lights for permission," the object of which is to entreat permission of the deity to regard him as freed from the divine displeasure, with liberty to his friends to renew their intercourse as before.

ART. 23.—*On Anomalous Exanthems.*

By Dr. BENJAMIN W. RICHARDSON.

(Proceedings of the Epidemiological Society: British Medical Journal, December 6, 1862.)

After narrating the various and serious difficulties which lie in the way of the practitioner in the determination of the contagious as distinguished from the non-contagious exanthems, Dr. Richardson proceeds to describe cases in which it was difficult to say whether the

disease was measles or scarlet fever. Having completed this survey, he asks: What is the nature of the cases to which attention has been directed? The answer resolves itself into one of the following propositions:—1. Either there is a distinct and contagious epidemic which is not isolated in our nosologies, which has its own poison and its symptoms, and with the positive identity of which the profession is not acquainted; or, 2. There is a combined form of disease, of which scarlet-fever and measles are the elements; or, 3. There is an unrecognised form of the disease scarlet-fever; or, 4. There is some known disease which assumes an anomalous character, simulating the combination of measles and scarlet-fever above named; or, 5. There is some unknown form of accidental disease from absorption of organic poison with which we are not familiar—the disease being idiopathic and non-contagious.

The author next proceeds to discuss these several propositions, with a view to eliminate such of them as did not answer to reasonable and fair scrutiny. After negating the first three propositions, he dwells at some length on the fourth, and discusses Dr. Ross's view that there is a form of urticaria which puts on symptoms that are confounded sometimes with measles, sometimes with scarlet-fever, sometimes with both; and which he regards as the source of many of the doubts respecting scarlet-fever. In reference to this view of the question, Dr. Richardson observes that, in the cases he has named, he does not think that urticaria was the misleading disease. If so, urticaria is open to a definition so wide under the term species, that our formula must be revised in regard to it. If that be urticaria which is *unattended* with wheals and with itching of the skin, but which is attended with sore-throat and ulceration, and universal redness of the skin, and which sometimes terminates fatally, surely the old reading of urticaria must be replaced by one more distinctive in form and more extensible in symptoms.

Dr. Richardson next discusses the last issue: Is there some distinct disease arising from organic poison which is not yet recognised, and which is the source of all our difficulties? He believes that there is, and he is of opinion that it has the following distinctive forms. It is an eruptive disorder, in which the skin and mucous membrane of the stomach and alimentary canal mainly, and perhaps exclusively, share. The disease has its origin in the alimentary tract, and is either primarily or secondarily connected with derangement of the nerves of organic life. The surface eruption, both on the skin and mucous membrane, is due to a loss of the controlling influence of nerve over blood-vessel. The disease is not contagious (the poison being fixed in character), except by direct inoculation. The disorder shows no tendency to produce disease of the kidney or uræmia.

Unlike scarlet-fever and measles, it is variable in its course; it may terminate at once, and favourably, by active purging or vomiting, by which means offending matters are thrown out of the canal; it may continue until it terminates in death.

It is probably most common in persons disposed to rheumatism.

The prime seat of the disorder seems to be in the alimentary canal. In the irregular digestion of some particular forms of food,

some product, probably of the character of a non-volatile acid, is yielded, and, being absorbed from the canal into the blood, is the poison on which the symptoms depend.

From the disease not being contagious, and from the fact that it arises in the body from malassimilation, it may be very appropriately named *idiopathic rosalia*.

ART. 24.—*Two Cases of Murrain (aphtha epizootica) in Man.*

By Mr. J. B. HISLOP, with Remarks by Dr. G. W. BALFOUR.

(*Edinburgh Medical Journal*, February, 1863.)

"There is," Dr. Balfour says, "no question but that the vesicular murrain is contagious, because it is well known to be inoculable, and whole herds have been often artificially inoculated with the view of giving them a milder disease, and getting them more rapidly through it; and, considering that this is truly a panzootic disease affecting animals of every class, cattle, sheep, deer, pigs, dogs, cats, horses, geese, ducks, pigeons, fowls, hares, rats, &c., it is surely more consistent with reason to suppose that the contagion has, even in the most obscure case, been conveyed to the animal by some dog or rat, than to imagine that it forms a mysterious exception to a well-known law. Dr. Hislop's two cases corroborate the fact that man forms no exception, but is equally susceptible to this contagion with other animals. Continental medical men have long been well acquainted with this fact, and numerous instances are found recorded in their journals. The disease had scarcely made its appearance in this country when it was found affecting man in the person of a young farmer, who was inoculated by means of a wound on his finger, and who had the characteristic vesicular eruption on his mouth and nose, attended by very severe constitutional disturbance and great prostration of strength. A similar but somewhat milder case was observed about the same time by Mr. Duncan, of Colinsburgh, in Fife, in the person of a boy employed in attending on diseased cattle. Many other cases have been since recorded; and they are indeed so numerous when ferreted out, that there is scarce a cattle-dealer who has not either had the disease himself, or knows of some who have had it. Fortunately in this form it is chiefly found to affect robust men; and it is probably never fatal, though it is just possible that some cases may be buried under the name of *diphtherite* in the books of the Registrar-General.

"But there is another and a much more insidious manner in which the contagion of this disease may be communicated to man, and in which it may prove unquestionably more injurious: first, because it is thereby more especially conveyed to those who are young and weakly, and who are thus less able to stand it; and, secondly, because the symptoms produced are apparently much less distinctly referable to the actual cause—I refer to the use of the milk of the diseased animals. Whether the milk of animals thus diseased is

injurious or not has been already the source of much controversy both at home and abroad; but in this, as in every other similar question, a few positively affirmative cases outweigh whole bushels of negative ones. "Tous les veaux nourris par des vaches ayant des aphthes aux mamelons ont péri," says M. Binet. Andreae (op. cit.) relates many instances in which calves, pigs, and dogs, fed with murrain milk, died from diarrhœa, accompanied by convulsions, and several cases of death among calves from the use of the milk of diseased cows are mentioned by R. H. Watson of Kelso, in the *Edinburgh Veterinary Review* for August, 1862, p. 505. All these animals were found on dissection to labour under gastro-enteritis; and this was also found to be the case in many pigs killed at once on being observed to be ill, as mentioned by Mr. Thombs. Dr. Hislop mentions that the children of the farmhouse he refers to suffered from sore-throat. Mr. Watson mentions that several of the farm-servants' children suffered from sickness, pain in the bowels, and considerable diarrhœa, which were cured by a dose of castor-oil, and stopping the diseased milk, the use of which, if persevered in, might have carried them off as well as the calves. Kolb says—"Lac potum in infantibus pluribus vomitum acidum et diarrhœam ciebat." Krügelstein mentions that children who drank murrain milk were seized with violent fever, swelling of the face, and vesicular eruption in their mouths. Mr. Gamgee mentions the death of eight out of nine calves from inflammation of the throat and alimentary canal; Erdt also mentions many instances of disease arising in men, and particularly in infants, from the use of murrain milk; and innumerable other instances might be given in which disease, chiefly in the form of gastro-enteritis, was produced in men, but particularly in children, by the use of butter, curds, or milk, from diseased animals, many of which might have proved fatal but for the timely cessation of the use of the milk. And yet it is indubitable that murrain milk must have been often used in various forms without anything more than, at the most, temporary inconvenience; and this is very easily explained: for when the disease is at its height the milk is apt to be rendered useless by the admixture of blood, or to be so much altered in appearance, or lessened in quantity, as to be rendered unusable, or at least to require large dilution before being sold; and dilution, of course, lessens very much the virulence of the poison. That the milk is really injurious is proved not only by the cessation of the symptoms on its use being discontinued, but also by their immediate production by the experimental drinking of the milk, as has been proved by Jacob and Hertwig."

Dr. Hislop's account of his two cases is as follows:—

"About the end of August last, Mrs. X., the wife of an extensive farmer, came under my care on account of an eruption of bright red spots, one-eighth of an inch in diameter, covered with a thin white desquamation, which were so densely sprinkled over her feet, legs, thighs, and the lower part of her body, as to leave only minute interspaces of sound skin. As Mrs. X. had within the last three years suffered from several attacks of hepatitis, followed by jaundice, and consequently was extremely liable to derangement of her liver and

digestive organs, I concluded that the eruption just described might be thus originated, and I prescribed for her alterative purgatives, along with five grains of bromide of potassium three times a day. This treatment made no impression on the symptoms.

"On a subsequent visit to my patient I found her husband complaining of sore mouth and throat. Upon examination I found the mucous membrane of his lips, mouth, tongue, and throat studded with small ulcers giving off a white slough, which left behind it a clean but highly sensitive cup-shaped cavity; his forehead was also covered with an eruption similar to that upon the lower extremities of his wife. As this peculiar combination of symptoms in parties so closely connected was, to say the least of it, remarkable, I made strict inquiries, and distinctly ascertained that the only cause that could be assigned for this peculiar affection was the circumstance that the whole of Mr. X.'s cows were at that time labouring under the vesicular murrain (*optha epizootica*), a highly contagious cattle distemper, in which the mucous membrane of the lips and mouth, particularly on the under surface of the tongue, is covered with vesicles and ulcers analogous to those by which Mr. X. was affected. Mr. X. informed me that while examining one of his cows which was suffering much from this disease, and while in the act of pressing back its lips, he observed two or three pimples on the upper lip to burst and eject the matter to a considerable distance, and that he received a portion of this matter on his hands and cheek.

"Knowing the high moral respectability of the parties, and having carefully weighed all the circumstances and peculiarities of the case, I came to the firm conclusion that Mr. and Mrs. X. had been independently inoculated with matter discharged from the apthous sores of the cattle; and this opinion was very much strengthened by my subsequently ascertaining upon inquiry that various other individuals employed about the cattle had suffered from similar symptoms, though in a less degree.

"I now put both my patients under the internal administration of corrosive sublimate and iodide of potassium in solution, and touched the ulcers on Mr. X.'s mouth, throat, and tongue, freely and frequently, with nitrate of silver, at the same time ordering the frequent use of a chlorate of potash gargle.

"Under this treatment the disease gradually but steadily declined. During its continuance Mrs. X. had a smart attack of conjunctivitis in both eyes, which yielded to the employment of the bichloride of mercury, collyrium, and covering the lids with the belladonna ointment.

"I may mention that the eruption upon my patients was never observed to be vesicular, as it is in cattle; the spots made their appearance as slightly elevated reddish prominences (*papule*), which gradually became bright red, then threw off a thin white silvery-like scale, and again gradually disappeared.

"The cases were indubitably independent of each other, as far as the contagion is in question; both parties, as well as all those affected in a lesser degree, had gone freely about the byres handling the diseased cows, of which there were at times upwards of a dozen

ill at once, and the peculiar virulence of the symptoms in Mr. X.'s case seemed to depend upon the circumstance that the matter caught upon his hand and cheek was derived from one of the worst of the diseased animals. Mrs. X. had slight inflammation of the fauces, which yielded readily to the application of nitrate of silver and the use of chlorate of potash as a gargle. Several of the children about the house were also affected with sore throats, but the symptoms in their case were mild and easily overcome. Mrs. X. and family were in the habit of using the milk freely fresh from the cows, though she did think 'it could not be good;' what remained was churned and sent to market. On talking over the matter with Mrs. X., I ascertained that the only way in which the introduction of the disease among their cattle could be accounted for, was the fact that they had allowed them to be fed upon clover which grew two miles and a half from their own byres, in close proximity to a field in which the cows of a neighbouring dairy were grazing, the said cows having the disease very bad at the time; and they therefore thought that the disease was brought in the clover carted from this field two miles and a half distant. The disease was not known to exist nearer, and no other mode of conveying the contagion could be discovered. I may add that I have observed many cases of herpes (ringworm), caught from diseased cattle, and I doubt not but that a better acquaintance with the diseases of our four-footed friends would reveal the source of many otherwise obscure affections in man."

(C) CONCERNING CHRONIC DISEASES.

ART. 25.—*On the Treatment of Anasarca and Ascites.*

By Dr. J. BIRKBECK NEVINS.

(*British Medical Journal*, December 18, 1862.)

Dr. Nevins advocates the importance of removing the fluid by means of acupuncture or tapping at an early period, instead of looking upon this plan of treatment as the last thing to be resorted to. He says:—

"I have frequently observed the difficulty experienced in obtaining the free action of diuretics in cases of anasarca, until the fluid has been removed by some other means, after which the diuretics have produced the desired increase of urinary secretion; and, as a consequence of this, the further accumulation of fluid in the cellular tissue has been prevented. The means by which the fluid has been removed in the first instance have sometimes been copious sweating in a steam-bath, sometimes free purging by hydragogues, and at other times acupuncture of the legs and feet; but in whatever way the object has been accomplished, the effect has been the same—viz., the natural action of diuretics, which had previously failed to operate.

"In looking for an explanation of this phenomenon, I have come to the conclusion that the secreting portions of the kidneys are so enfeebled in their action by the pressure of the accumulated fluid

present throughout the cellular tissue of the body, that they are unable to perform their duties aright, until the pressure is removed by some of the various methods just mentioned. The steam-bath or hydragogues would naturally be tried in the first instance; but if they do not *speedily produce the desired effect*, I urge the early employment of acupuncture, and think the results of my experience are now sufficiently extensive to warrant my bringing the subject thus prominently before my medical brethren. I have for some years laid this principle before my class in lecturing on diuretics at the Medical School; but I have waited for accumulated experience before making more public mention of it.

"In addition to its effect in promoting the action of diuretics, I consider the early employment of acupuncture important with reference to the condition of the patient's skin. When it is employed early, and before the skin has become enfeebled by the great and long-continued distension, I have never seen any bad consequences follow the operation; but when it has been delayed to the last moment, and the legs have long been swollen to a painful extent, I have seen inflammation follow the operation, which has been troublesome in more than one instance.

"In performing the operation, I advise that a few punctures only should be made—say half a dozen in the first instance—in one leg and foot; and that the patient should afterwards sit up as much as he conveniently can. The limb should be folded in flannels, which should be frequently changed as they become wet; and it is frequently necessary to apply warmth to the foot in bed. I recommend the employment of a common *stout* round sewing needle, in preference to a cutting edged needle, which I have for some time ceased to employ, as I have seen the wounds made by a three-cornered needle followed by inflammation more frequently than those made by a round one. The round punctures do not allow the escape of the fluid quite so freely as the others; but the comparative freedom from subsequent inflammation is important; and the operation with a common needle supplied by the patient himself, appears much less formidable to him than when the instrument is taken from the surgeon's instrument-case. As soon as the oozing of fluid ceases, the operation should be repeated if the swelling still continue.

"With reference to the treatment of *ascites by early tapping*, I have less extensive experience; but the following cases appear to me to hold out sufficient encouragement to warrant the fair trial of any plan which gives us reasonably increased hopes of cure in so generally hopeless a disease as dropsy of the abdomen. When the ascites is apparently dependent upon simple disease of the serous membrane (as, for example, in hydrocele), I think we have considerable encouragement in hoping for recovery; and even if it is due to cardiac obstruction, or to disease of the liver, I should still adopt the same mode of treatment, because it offers tangible advantages to the patient, with no corresponding disadvantage, so far as my experience extends.

"It is familiar to us all how difficult it is to obtain the good effects of elaterium, in consequence of the vomiting which it excites; and,

owing to this circumstance, we are frequently obliged to resort to tapping merely as a palliative, to relieve the urgent sufferings of the patient. Now, if the delay of the operation enabled us to make one tapping suffice for cure, we should be warranted in urging our patient to bear his sufferings, in the expectation of radical relief; and he would be repaid for his patience. But, since we find that repeated tapplings are the rule, and not the exception, it appeared to me very doubtful whether, after all, the benefits from delay did repay the prolonged distress from waiting; and I, therefore, repeated the tapping as soon as the patient became uneasy, without waiting for positive suffering.

"I have further observed that, in some cases, hydrogogues acted upon the bowels after the patient had been tapped, although they had produced vomiting, or failed to purge before the operation; and I venture to suggest, therefore, that in this case also, the removal of pressure enables the organs to perform their functions, which they were previously unable to do.

"I have now adopted this plan of treatment in three cases of ascites, which have resulted in perfect cures. The first case was that of a woman in whom the early tapplings were delayed in accordance with our usual practice, and she was tapped many times before the fluid ceased to accumulate; but at length she was discharged cured, and died eighteen months afterwards of some other disease. The second patient was a man, who recovered after three tapplings; and the third was a woman, who also recovered after three tapplings.

"I regard *constant mechanical support of the abdominal walls as a very important part of the subsequent treatment*, by enabling the tissues to regain their natural tone and vigour, after the extreme and enfeebling distension to which they have been so long subjected; and I therefore strictly enjoin the constant use of a flannel abdominal bandage, which I do not allow to be given up until the fluid has ceased to reappear.

"In the course of *one or two days after the operation, I always resume the use of elaterium or jalap* in such doses as the stomach will bear without sickness, which ought always to be carefully avoided; and I have frequently found that a single pill daily, containing one-sixteenth of a grain of elaterium, produces copious watery purging without sickness after the operation, though the patient derived no benefit from it before the tapping.

"The health should be sustained by a liberal diet, of which malt liquor or spirits may form a part or not, according to the circumstances of the case. Iron tonics have also appeared to be useful."

ART. 26.—*A New Wrinkle in the Treatment of Gout.*

By a Hospital Physician.

(*Medical Times and Gazette*, July 19, 1862.)

"On the morning of April 6th," says our anonymous writer, "I was awakened with severe pain in the dorsum of the left foot, which increased in intensity during the day, prevented sleep during the

night, and increased to such an extent on the morning of the 7th, that I could no longer doubt that I was 'in' for my first attack of gout (hereditary) at the age of forty-two. As I lay vainly seeking for an easy position, my mind was busy analysing the seat and nature of the pain. It resembled the pain of a very severe sprain I had had years before in the same foot. After a while some comfort was gained by placing the sound foot behind the lame one, so as to support it; but sleep was impossible, as after the sprain, for no sooner did oblivion touch the senses, than a fearful twitch awoke them to life. To relieve the member from the weight of the bed-clothes, an ordinary arch was adopted, but the amount of comfort it produced was small.

"The 8th found me in greater suffering than ever. My mind had now come to the conclusion that the pain was due to the stretching of ligaments, of tendons, of muscles, and of muscular fibres. The swelling of the foot and the redness of the skin showed the presence of inflammation, and I was familiar with the exquisite sensibility of all inflamed fibrous tissues. This stretching was readily accounted for, as it is clear that in the ordinary position of the foot in bed, especially when pointed upwards, there is much leverage on every joint anterior to the heel, and also that the posterior muscles, being more powerful than the diseased anterior ones, tend directly to elongate the former. The veins, too, about the foot were swollen; they are fibrous, and very little distension of their calibre by coughing, blowing the nose, or hanging the foot down increased the general pain. Perfect rest, when it could be got, was pleasant, and none was such complete rest as that obtained by a gentle hand behind the 'sole,' the toes at the time pointing upwards. From these considerations, I got a high hassock in the bed, against which I could rest the sole of the foot, and I then found the supine posture preferable to any other; but even when lying on my side, I found it a comfort to have the 'sole' supported. Late on the 8th, the intense pain began to subside, and the opiates, which were useless before, procured sleep. On the morning of the 9th, the foot was swelled, angry-looking, red, exquisitely tender to the touch, but comforted by pressure and gentle rubbing, and as helpless as a log; every attempt at moving it was stopped by pain. I was confined entirely to bed, for pain prevented me using the leg at all; but while at rest I felt nothing wrong. If, therefore, it occurred to me, I can keep in this quiescent state perpetually, I shall gain some comfort. After turning over in my mind many contrivances, none seemed to promise better than the old-fashioned strapping with strips of adhesive plaster. I had adopted the plan not long before in an hospital patient who had a gouty knee, and with apparent success; and by the same means I had cured many very acute pains in other parts of the body.

"I soon got the plaster, duly cut it, and then made the trial, being my own surgeon. The placing of the first few strips was very painful, as on each occasion I had to raise the foot from the bed, but ere the fifth was placed the comfort began to be marked. Each strip subsequently increased my powers, and when at length I had the

member encased from the toes to four inches above the ankle, all pain had gone, except when any great strain was laid on the foot. In a few minutes I was out of bed, with a light chair as a support for the knee of the lame leg, and with a slipper on the other, making a tour to the neighbouring room-windows to inspect some horticultural proceedings. Shortly afterwards I used an enema comfortably, having been in too great suffering previously to venture on any movement of the bowels. (How I should have anathematized a doctor, had I been a layman, who had treated me with purges!) In fine, I was able to do anything but walk or stand on the leg. The night I spent was a pleasant one, and on the morning of the 11th the swelling of the foot had gone down so much that I had to renew the strapping. On removing the strips, the pain was almost as bad as it had been the day before, but the inflammatory redness had all subsided, being replaced by a purplish hue. The strapping having been replaced in such a manner that the sole of the foot is at right angles with the leg, I am now able to walk slowly with the aid of a stick—a feat which I dared not, a few days ago, hope to attain to for some weeks to come.

“Now, I believe that the above plan is a novelty. I doubt whether I dare have recommended it to another's foot; I know that I did not do so to my father, and that my own wife urgently recommended me not to use plaster to such an angry skin as mine, or to such an inflamed foot. As a novelty I introduce it, but back it with the testimony of my own personal experience. The completeness of the relief I can only compare to that following the evacuation of a painful abscess. Of its verity any one can judge by taking the straps away for a while, and then replacing them.”

SECT. II.—SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 27.—*On the Treatment of Delirium Tremens.*

By Prof. LAYCOCK, of Edinburgh.

(*Edinburgh Medical Journal*, November, 1862.)

In an excellent paper on the diagnosis, prognosis, and treatment of delirium tremens, Professor Laycock again advocates the expectant and rational mode of treatment. “Since a case of delirium tremens tends,” he says, “independently of active remedies, to a favourable termination in from four to fourteen days (the cases I have treated have averaged six days' duration), the great indication of medicinal treatment is to favour this tendency in *expectation* of early recovery. It is favoured by preventing as well as by helping. The natural impulse to interfere by the aid of narcotics and stimulants, or by mechanical means of restraint, has to be checked. This is best attained by adopting a plan of treatment which occupies and gives confidence to the attendants and friends, and at the same time calms the patient. The effects of medicinal agents or drugs used to this end cannot be satisfactorily determined in many cases, because

we cannot say, when calm and sleep come on, how much is due to the drug, how much to the diet and regimen, and how much to nature ; so that all experience upon this point is somewhat doubtful. It is certain, however, that drugs have and do exercise an influence over the intensity of the symptoms, although they may not either cause sleep or shorten the duration of the disease. Of these, opium and its salts, tartar emetic, digitalis, chloroform, purgatives, alcoholic and other stimulants, are examples.

“Alcoholic Stimulants.”—These are available in all asthenic forms of delirium, however caused. They have been hitherto administered in the methystic form, chiefly on the theory that the sudden withholding of the habitual stimulant is the exciting cause of the delirium. The depression of the nervous system may be partly due to the want of the accustomed stimulus ; but all experience shows that it is still more commonly due to morbid causes of a more general character, such as induce a feverish cold, a fit of indigestion, of the gout, or the like. Without such concauses, abstinence from habitual stimulants will not excite delirium tremens. The habitual drunkard distinguishes the depression which commonly succeeds to stimulation as ‘the blues ;’ ‘the horrors’ is a different thing, and occurs when any indisposition induces loss of appetite, languor, disturbed sleep, and other symptoms of the class. It is the depression thus induced by this same morbid cause which constitutes the first stage or simplest form of delirium tremens. The intensity, therefore, is partly, at least, determined by the kind of indisposition or acute affection ; and it is this we have to remedy. The indications, therefore, for the administration of alcoholic or habitual stimulants must be drawn from the then condition of the patient, just as in other diseases in which remedies of this class are useful. When food has not been taken for several days, and the hallucinations are of a frightful or distressing kind, and especially when the pulse is very quick and feeble, the first sound of the heart heard indistinctly, the tongue coated, œdematous, and flat, or indented at the edges, wine and brandy may be administered medicinally with advantage. Sometimes this state of prostration is due to the combined influence of drinks and opium or its salts, or to opium alone. In either case, alcoholic stimuli may be given.

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“Opium and Salts of Morphia.”—The influence of these drugs is very various ; in one class of cases having the most beneficial effect, in another increasing greatly the excitement and delirium. The like difference in effect is seen when given in cases of melancholia and mania, for which they have been freely prescribed. In some of these, as in some cases of delirium tremens, very large, and, under ordinary circumstances, poisonous doses have little effect. This tolerance of opium in certain forms of delirium tremens has probably led to its heroic administration in cases generally. A question has arisen, whether, in those thus treated which terminate fatally, the death is due to the drug or the disease.

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“My own conclusions on this point are, that the combination of

alcoholic drinks with opium tends to render the patient more tolerant of the drug; that in some drunkards its operation is so much delayed, that when given in repeated doses, there is a cumulative effect produced; that it is never a wholly safe practice to administer it for the express purpose of procuring sleep, nor as a stimulant in more than the ordinary doses; and that it is always prudent to watch the effect of the remedy on the pupils in exciting contraction. How far various other states of the encephalon may antagonize the drug, and for how long, we never perhaps can say, but that there are such states variously induced is one of the most certain things in physic. We have it in cases of both mania and melancholia, in certain kinds of neuralgia, in traumatic tetanus; and it is believed that it may be induced by henbane, belladonna, and other drugs. And it is to be remembered that the antagonizing state may be so transient as to leave the brain exposed to the full action of the poison before it is eliminated—nay, by its action on other viscera may delay the elimination.

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“Camphor.”—It is not easy to determine beforehand when opium or its salts serve only to induce greater prostration and distress; most generally, however, the patient is of a nervous habit with a florid complexion, or at least has had, and is of a neuro-vascular diathesis. In cases of this kind, where the exhaustion is great and morphia inadmissible, camphor proves sometimes useful, in from two to three grains every three hours, or the carbonate of ammonia combined with camphor and henbane.

“Mental Hypnotics” are singularly successful in those cases in which there is a morbid apprehension as to sleepless nights, and a hypochondriacal anxiety for sleep. It is often the morbid feeling alone which prevents sleep: this is proved by the circumstance, as repeatedly witnessed in my practice, that any simple remedy administered to the patient so as to impress him with the conviction that it will cause sleep, is followed by sleep; and sometimes, when convalescence is approaching, by as prolonged a sleep as if a powerful narcotic had been taken. In one case of this kind the long sleep which followed upon a placebo excited alarm.

“Tartar Emetic.”—This drug, like opium, has been administered in large doses in delirium tremens; the effect of which, I venture to say, can only be to excite gastric or gastro-enteric inflammation. Now, it is probable counter-irritation of the gastro-intestinal mucous membrane is very beneficial in some cerebral affections. Perhaps calomel sometimes acts in this way in acute hydrocephalus; but in the majority of cases of delirium tremens this result is attained by a simple purgative, and care should be taken to avoid irritating the already irritated stomach. Tartar emetic, therefore, should be administered in solution, and in doses not exceeding thirty minims of the liquor of the pharmacopœia. It is chiefly indicated in those cases in which there is some inflammatory complication, and especially pneumonia, however trifling. It is advantageous, too, at an early period in those in which the whole character of the disease is more ethenic, and the mental disorder more nearly approaches insanity or mania. In these the patient is less apprehensive and

timid ; often loquacious, suspicious, and inclined to be aggressive upon slight provocation ; he has notional delusions more predominantly than hallucinations of the senses ; his nights are disturbed, but not wholly sleepless ; nor has he tremors of importance. His appetite comparatively with the asthenic form is little impaired ; his tongue but little coated, and when projected rather pointed and firm, than fat, flabby, and tremulous. His skin, too, is rather hot, or at least natural, than cool and moist ; and the pulse is less round, undulating, and quick. Cases of this kind are intolerant of stimulants and opium even in small doses, these being apt to change a 'cantankerous' kind of delirium into a raving or destructive sort ; whereas twenty to thirty minims of tartar emetic liquor, given every three or four hours, calm, or at least do not aggravate. When, however, there are symptoms of depression, especially in a young person, and the history is that of causes of exhaustion, laudanum in five to fifteen minim doses is a useful stimulant, in combination with fifteen to twenty minims of the liquor. This has long been found useful in ordinary maniacal delirium thus arising.

"Emetics, Purgatives, and Stomachics.—Methods of treatment by purgatives, quinine, and tonics have been recommended. The patient before coming under treatment has usually tried remedies of this class, especially *bitter* drugs, either as bitter tinctures, 'the bitters' of the dram-shop, or in bitter beers. The effect of these is to modify the symptoms, and especially to induce muscular twitchings, tremors, and even slight convulsive attacks. Very often in such there has been a total loss of appetite, and no food has been taken for several days. In some, food is vomited ; in others, if retained, it causes pain, because of the state of the mucous membrane of the stomach. This is often, in fact, congested and inflamed. Hence the dietetic and medicinal treatment of gastritis is indicated ; constipation and hepatic congestion are not uncommon complications, and indicate a suitable aperient. Podophyllin, calomel, colocynth, and henbane, castor oil, salts and senna, and Gregory's powder, were the ordinary remedies of this kind used in the infirmary. Of the new drug as a bilious purgative I can speak favourably. Podophyllin was prescribed in several cases in combination with cannabis indica or henbane, with good effect. The following formulæ were used :—R. Podophylli, gr. ij ; pulv. cinnamomi co., extract. hyoscyami, aa \mathfrak{z} ; mucil., q. s. The mass to be made into four pills, of which one to be taken every six hours until the bowels were moved. In another formula, gr. viij of powdered ginger and of extract of cannabis indica were combined with gr. iij of podophyllin, and made into six pills. The purgatives, of whatever kind, were always given at the commencement of the treatment, and not afterwards. Emetics were never tried, and were never indicated except in cases of drunkenness. Stomachics were prescribed in a few cases at the termination, when convalescence was established, but were rarely needed.

"Digitalis and Chloroform.—There is evidence of the calming effect of digitalis, but it is of the vaguest kind. There is no indication of the class of cases in which it may be safely prescribed, nor

are we clearly informed whether in the cases reported there was not renal or cardiac disease, or the complication of drunkenness or of narcotization. I have seen it tried in one case, in which, in consultation with a medical friend, it was resolved to try a half-ounce dose of the tincture. The patient had had a drinking bout, and suddenly became aggressive and destructive, tearing, pulling down, and burning, and striking and throwing things at the attendants. There was no loquacity: the patient rarely spoke, but sat in bed, rolling up the bedclothes, tearing off his clothing, and throwing food and drink in the faces of those who offered it. This he did with his dose of tincture of digitalis, after drinking one half of it. The case was one in which the expectant treatment was thereupon tried with entire success—reason being restored and convalescence established within the week. Chloroform has been administered in very violent cases with advantage: when exhaustion is likely to come on from the constant raving and struggles of the patient, it may save life by saving strength. Such, however, are rare, and are more frequently met with as the result of heroic treatment than in the ordinary course of the disease."

ART. 28.—*On the Influence of Cardiac Hypertrophy and Diseases of the Cerebral Arteries in the Production of Apoplexy.*

By Dr. A. EULENBURG.

(*Wiener Med. Wochenschr.*, September 6, 1862; and *British Medical Journal*, December 6, 1862.)

Dr. A. Eulenburg has investigated this subject statistically in a prize thesis presented to the Medical Faculty at Berlin. In 42 cases of sanguineous cerebral apoplexy, abnormal conditions of the arteries at the base of the brain—hardening, calcareous deposits, and fatty degeneration—were found in 29: in 13 cases only were the large cerebral arteries free from disease. In 9 of the 42 cases there was hypertrophy of the left ventricle. Of the 29 cases in which disease of the cerebral arteries was present, there was also more or less extensive endocarditis in 17, alterations of the valves of the heart in 19, and hypertrophy of the left ventricle in 6 only. Dr. Eulenburg hence draws the conclusion that disease of the cerebral arteries is a much more frequent cause of apoplexy than cardiac hypertrophy.

ART. 29.—*On the Curability of Abscess in the Brain.*

By M. FLOURENS.

(*Gazette Hebdomadaire de Médecine et Chirurgie*, November 28, 1862.)

In a recent communication to the Academy of Sciences at Paris, M. Flourens relates some extraordinary facts concerning the brain, resulting in part from his own experiments. He quotes several in-

stances, recorded in history, of cures effected, in cases in which the brain received serious injuries; among them that of a young officer in the time of the Fronde, whom Cardinal Mazarin had always refused to promote on the plea "that he had no brains." In one of the engagements of those stirring times, this young man received an enormous wound on his head. The surgeon in whose hands he was, astonished at the quantity of brain which issued from the wound, preserved it in spirits, and subsequently, after this patient had completely recovered, showed it to him. "Oh!" exclaimed the latter, "pray send that to the cardinal, to prove that I have more brains than he is aware of." Another case mentioned was that of a young man of sixteen, who had been struck by a stone on the left parietal bone. As the bone did not present any appearance of fracture, the treatment employed by the surgeon, Lapeyronie, was of the simplest kind. But on the 25th day, the patient's right eye began to grow weak, and three days later its power of vision had ceased, the patient himself being in a state of absolute prostration. Lapeyronie made several incisions on the skull, which he trepanned three times; the dura mater was relieved of a few splinters which pressed upon it, after which he opened it, and a quantity of purulent matter issued forth. Immediately the prostration ceased, and the patient recovered his eyesight and the complete use of his other senses. At the end of two months he recovered his health entirely, although he had lost a considerable quantity of brain. M. Flourens then recalls to mind several experiments of his own on animals, which he deprived in some cases of one lobe, and in another of both lobes of the brain, the animal living upwards of a year after the operation; but having lost all its senses, and being reduced to the state of a mere automaton. In another instance the whole cerebellum was extracted; the animal lived more than a year after the loss, but never recovered the regularity of its movements, being reduced to a permanent state of apparent drunkenness. M. Flourens next proceeds to describe certain new experiments, in which he introduced leaden bullets into the brain of rabbits and dogs. The bullets were placed on different points of the upper region of the encephalon, on the lobes, and the cerebellum. The bullets, left to the action of their own weight, penetrated by degrees into the substance of the brain, and ultimately stopped at the basis of the cranium. The passage thus opened through the substance soon closed and healed; and if the bullet was not too large, none of the regular functions of the animal were disturbed, and no inconvenience whatever was produced.

ART. 30.—*On Cysticerci of the Brain, and their Diagnosis.*

By DR. GRIESINGER.

(*Archiv. d. Heilkunde*, 1862; and *Med.-Chir. Review*, January, 1863.)

After collecting together between fifty and sixty cases from available sources, including two of his own, the author seeks to establish data by which, in his opinion, this affection may be recognised

during life. He divides the various collected cases into five categories. 1. Those which run their course without any, or with but very obscure symptoms. 2. Cases in which epilepsy exists without mental disturbance; in these cases death occurs either independently of epilepsy, or so far connected with it that the epileptic symptoms first set in shortly before death, or the attacks increase exceedingly shortly before death, or a kind of cerebral disturbance is developed from one or more of the attacks which leads to death. 3. Cases in which, along with epilepsy, a psychical disturbance is at the same time set up, whose continuation and character warrant the designation of a mental disease. The psychical disturbance is expressed at one time as mania, true delirium or confusion of mind, at another as obstinacy or imbecility, and appears either before or after epilepsy. 4. Cases in which epilepsy is wanting, but in which the mental disturbance exists, accompanied frequently by other motor and sensitive disturbances. In some of these cases other pathological changes within the cranium appear to form the basis of the psychical disease. 5. There are cases in which neither epilepsy nor mental disturbance exists, but cerebral symptoms of irritation or torpor, which come on shortly before death, or assume a chronic form.

The cysticerci were situated chiefly at the periphery of the brain, in the grey cortical layer; whence it is inferred that they had not been themselves carried thither, but that their germs had. The epilepsy from cysticercus is in all respects like cerebral epilepsy, and the psychical disturbances in general have nothing characteristic about them. Very often also other cerebral symptoms coexist, such as squinting, alteration in the pupils, avoidance of light, headache, coma, anomalous sensations in the limbs, &c.

The following propositions are set forth as results of our author's experience:—

1. The diagnosis of cysticercus must be based on a twofold series of considerations: on the one hand, resting on the improbability of any other cerebral affection, the symptoms not corresponding; and on the other, on the presence of definite series of symptoms.

2. Cases of convulsive attacks being more or less epileptic, are chiefly suspicious, which come on in a subacute way, or quickly increase to a certain pitch, and presently, after a steady increase in number and intensity, assume the general appearance of a very severe cerebral disease.

3. The probability of cysticercus is increased if these symptoms come on in patients of adult age, being previously healthy, or in men in whom neither hereditary disposition nor traumatic or syphilitic influences, nor lesions of the vessels or heart, could have given rise to them.

4. A suspicion of this disease would be aroused if, under the circumstances last named, mental depression and confusion, accompanied with giddiness, loss of sight and hearing, headache, coma, &c., occur.

5. Symptoms of cerebral lesion, if conjoined with paralysis, are to be looked upon almost with certainty as not having origin in the cysticercus.

6. The manifestation of cysticerci in external parts under such circumstances naturally elevates the probability into certainty.

ART. 31.—*On the Pathology of Insanity.*

By Dr. JAMES GEORGE DAVEY.

(Pamphlet. London. 1853. Pp. 16.)

This pamphlet is a reprint of an article which appeared in *The Zoist* as far back as July, 1843. It shows very plainly that Dr. Davey enunciated these opinions full twenty years ago, and so far makes good a claim to priority on the part of its author.

"I am disposed," says Dr. Davey, "to regard insanity as of two kinds—the one dependent on *nervous irritation of the brain*, and the other on *inflammation*. The very common indications of the existence of past or present inflammatory action of the brain or membranes, I consider a proof of not only the occasional associations of diseased cerebration with inflammation, as its immediate cause, but also of the frequent occurrence of such in the progress of insanity—that is, of that form of the disease consequent on 'nervous irritation.'

"The patients in Hanwell are very liable to attacks of cerebral and meningeal inflammation, and which not unfrequently prove the immediate cause of death. In such cases the general symptoms which indicate the existence of inflammatory disease assume the same asthenic character which belongs to peripneumonia, enteritis, erysipelas, &c. &c., when occurring in nervous and irritable subjects. Upon the same principle that such persons are more liable to the more ordinary derangements of the general health, of which chronic inflammatory diseases form a great part—so are the insane predisposed to the occurrence of cerebral and meningeal inflammation, and hence the ordinary appearances observed after death.

"The origin and progress of many cases of insanity are sufficient to prove this position: *e.g.*, suppose, for the sake of illustration, that an individual of delicate fibre is suddenly frightened by some cause or other, and, instead of her recovering from the consequences of alarm, they continue with aggravated severity. The faintest sound which reaches the ear is construed into a renewal of the first cause of her deep affliction; the gentlest wind which may happen to blow seems to threaten her yet more sorely. Every surrounding object at length appears tinctured with the cause of her misery, and each effort of herself and friends to shake off the horrid incubus is vain. Time rolls on only to show how much she is the instrument of her involuntary feelings. At length the judgment is betrayed into acquiescence. She no longer merely feels her sufferings, but she seeks a cause for them which shall not only excuse them to herself, but be in strict harmony with her predominant feelings; and thus, in passing from bad to worse, she at length realizes the precise condition of one labouring under acute mania. The deranged cerebration (insanity) is, in such a case, necessarily the effect of an

irritation of the ultimate fibrous structure of the brain, and which must be regarded as the consequence of the application, through the medium of the external senses, of a stimulus so intense as to prove incompatible with the healthy physical capacities of the organ. That a similar abnormal effect results from the application of a stimulus very much less concentrated, so to speak, if it be permanent, is quite certain.

"Again: if we imagine an individual labouring under intense avarice, grief, or pride, it would follow that the increasing physical action of the same portion or portions of cerebral substance would tend to the development of such a state of susceptibility and irritation of the parts concerned, that at length the volition would become suspended; or, in other words, the morbid action would acquire so great a supremacy as to subjugate every other feeling and propensity, and which of course must be, as above asserted, incompatible with the healthy physical capacities of the brain. Under such circumstances, the cerebrum may be compared to any ordinary muscle which from long use has acquired the habit of executing a certain movement involuntarily, although perhaps it may be painful or disagreeable.

"Now if such an abnormal state (irritation) of the cerebrum remains unrelieved, nothing is more likely than the occurrence of inflammation of the brain and its membranes, more or less insidious; and which progressing, would necessarily induce those palpable disorganizations of structure, effusions, &c., so generally observed. Such, I repeat, are generally the effects of disease, and not its first cause.

"In this light, then, it is seen that I consider insanity to be essentially a nervous disease, and the consequence of an irritation of the ultimate structure of the brain, consisting in a neuralgia of the sensory fibres.

"Insanity, like other nervous diseases, when not depending on local inflammatory action, is invariably aggravated by general bleeding: the exceptions to this rule are in the cases consequent on meningeal or cerebral inflammation, whether or not dependent on local injury. What very materially confirms this position is the fact, that the most violent forms of furious mania most commonly occur in persons of weak and delicate fibre and great susceptibility. I frequently witness the most urgent symptoms of acute insanity in combination with a small and feeble and quick pulse, cold skin, and a retracted and anxious countenance, &c. Neuralgic and nervous diseases generally are for the most part associated with similar constitutional symptoms. And, moreover, the most appropriate and successful treatment, in both instances, consists in the administration of sedatives, with a generous diet, and the employment of those various means calculated to improve the general health. Many cases of violent mania are cured at Hanwell by the administration of wine and steel.

"I mention this, of course, only in support of the pathological views."

ART. 32.—*On the Symptoms of Softening of the Cerebellum.*

By Dr. E. BROWN-SÉQUARD.

(Lancet, December 28, 1861.)

After relating several cases of softening of the cerebellum, Dr. Brown-Séquard calls attention to the extreme differences presented by them. He says:—

“Not two are alike; but, although differing one from the other, they concur in showing that the inflammatory softening of the cerebellum differs notably from the same affection of the cerebrum. Excepting one case (1, Woillez) in which there had been some temporary pains in the lower limbs, and another (8, Andral) in which fornication had been felt—most likely depending on the alterations of the crura cerebelli—nothing in these cases had been noted similar to the referred or local sensations or pains in the paralysed limbs which are so characteristic of the inflammatory softening of the cerebrum. The various kinds of involuntary movements (such as cramps, twitchings, jerking, trembling), which are prominent symptoms of inflammation of the cerebrum, are also almost completely missing in the above cases of cerebellar softening. In only one case (9, Andral) a contracture was observed. The pulse also, except in a few cases, did not exhibit the irregularity in rapidity and strength which are to be observed in cerebral inflammatory softening. The only symptoms that were noted several times in the above cases were:—

“1st. *Headache*.—In all the cases recorded with details, except two (1, Woillez; 10, Andral), there was a fixed pain in the back of the head, and generally in the side of the alteration in the cerebellum.

“2nd. *Amaurosis*.—In two cases (4, Duplay; 8, Andral) it was complete in both eyes, although one half alone of the cerebellum was altered; in another case (11, Andral) it existed in the right eye, while the left side of the cerebellum was alone altered.

“3rd. *Hemiplegia*.—This was complete in two cases (7, Lallemand; 8, Andral), and incomplete in one (1, Woillez). In one case the left arm alone was paralysed (4, Duplay). That these symptoms are not due to a *loss of function* of the cerebellum is proved by the fact, that in many cases of softening of one lobe of the cerebellum (as well as of other affections of that organ) there was no paralysis.

“4th. *Paraplegia*.—In two cases (3, Bianchi; 7, Lallemand) there was a paralysis of the lower limbs. This is not also a symptom of loss of function of the cerebellum, as it does not exist even in cases of a much more complete alteration of the cerebellum.

“As regards the other symptoms observed in the above cases, the most interesting were—a tendency to walk backwards (2, Binard); a tottering gait (1, Woillez; 2, Binard); vertigo or giddiness (1, Woillez; 8 and 9, Andral); an emotional and a semi-convulsive agitation of limbs (3, Bianchi; 11, Andral); audition obtuse (4, Duplay); aphonia (1, Woillez).

It is worthy of remark that vomiting, which is a frequent symptom

of other diseases of the cerebellum, has not been noted in any of the above cases. A facial paralysis has been observed in one case in which there was a complication (7, Lallemand). There was no anæsthesia except in one case (1, Woillez), in which it was incomplete, and probably depended upon some unseen alteration elsewhere than in the cerebellum."

ART. 33.—*On the Diagnosis of Hæmorrhage into the Cerebellum.*

By Dr. E. BROWN-SÉQUARD.

(*Lancet*, November 2, 1861.)

The following table will show at a glance that there are striking differences between the symptoms of hæmorrhage in the cerebellum and those of hæmorrhage in the other parts of the encephalon, not including amongst them the pons Varolii and the medulla oblongata :—

Table of Symptoms of Hæmorrhage in the

<i>Cerebellum.</i>	<i>Cerebrum.</i>
1. <i>Coma</i> rarely very deep.	Less rarely very deep.
2. Frequently no decided <i>paralysis</i> .	Almost always a <i>paralysis</i> .
3. Great <i>weakness</i> in every part of the body.	<i>No part very weak</i> , except, of course, the paralysed part.
4. <i>No facial paralysis</i> .	<i>Facial paralysis almost constant</i> .
5. <i>Loss of expression</i> of the face.	<i>No marked loss of expression</i> , except as a consequence of the drawing of the face on one side.
6. <i>No deviation of the tongue</i> .	Tongue very frequently deviated.
7. <i>Loss of speech</i> not frequent.	Frequent.
8. No paralysis of muscles of the eye.	Such a paralysis not rare.
9. <i>Pupils generally affected</i> , sometimes contracted, sometimes dilated.	Pupils rarely affected.
10. <i>Anæsthesia</i> very rare.	A slight anæsthesia frequent.
11. <i>Hyperæsthesia</i> frequent.	Very rare.*
12. <i>Amaurosis</i> not very rare.	No amaurosis, unless the hæmorrhage takes place in or near the tubercula quadrigemina or some other parts of the base of the brain.

* By the word hyperæsthesia, Dr. Brown-Séquard means simply an increase of the power of feeling, and not the existence of pains.

Cerebellum.

13. Excepting sometimes *tinnitus aurium*, no trouble in the senses of hearing, of taste, and of smell.
14. *Convulsions not rare.*
15. *Vomiting very frequent.*
16. Breathing generally very difficult.

Cerebrum.

- Tinnitus aurium*, and diminution of the power of hearing frequent.
- Rare.*
- Rare.*
- Generally not so difficult.

In the above list of symptoms, the most important are the facial paralysis, the loss of speech, and vomiting. In the cerebellar hæmorrhage, the first two will not be observed, and the last will generally exist; while in the cerebral hæmorrhage the reverse is the rule: generally no vomiting, and usually a facial paralysis, with a more or less complete loss of speech. Moreover, cerebellar hæmorrhage is generally fatal before a long time; while cerebral hæmorrhage is not frequently fatal in a short time.

The generation, or mode of production, of the symptoms in cases of hæmorrhage in the cerebellum is a most important subject of study to the scientific physician. The three modes by which symptoms are produced in cases of disease of the nervous centres—i.e., 1st, a loss of function of the part injured; 2nd, the effects of the irritation of that part; 3rd, the effects of pressure on neighbouring parts—have a share in the generation of symptoms in cases of cerebellar hæmorrhage. This will be clearly proved by a special examination of the mode of production of each of the symptoms.

1. *Coma*.—This symptom is evidently an effect of pressure upon the brain in cases of hæmorrhage in the cerebellum, as well as in cases of hæmorrhage taking place anywhere else inside of the cranium. As a general rule, the larger the amount of blood the deeper is the coma. This rule, however, is liable to exceptions, as a comatose state has sometimes been produced by a small amount of blood; while, in other cases, a larger quantity has not produced coma. One of the causes of this exception is certainly that in some cases the hæmorrhage is rapid, and in others it is slow; and it is well known that pressure slowly coming on is borne easily by the brain.

2. *Disorder of voluntary movements*.—Were the lobes of the cerebellum the centres that regulate voluntary movements, or the *central seats of the muscular sense*, as admitted by Professor Carpenter and Mr. Robert Dunn, we should find a paralysis of the muscular sense, or a disorder of voluntary movements, in those cases of hæmorrhage destroying the greatest part of one of the lobes of the cerebellum. Such a disorder has been observed in a few cases, but it has not existed in many others. The existence of a tottering or disordered gait in a few patients after a hæmorrhage in one of the lobes of the cerebellum might certainly be considered as countenancing the view that these lobes have some special function relative to the direction of voluntary movements; but the fact that the irregularity of the voluntary movements in those cases exists in

both sides of the body, instead of in one half only, clearly proves that the cause of this irregularity is not in the alteration in the cerebellum, as sometimes this alteration existed only in one half of that organ. Elsewhere Dr. Brown-Séquard shows by the evidence of a great many cases, that the disorder of voluntary movements sometimes observed in cases of morbid alterations of the cerebellum, depend upon the pressure on the pons Varolii, and not upon a loss of function of the cerebellum.

3. *Hemiplegia*.—In cases of hæmorrhage in the cerebellum, where there is a hemiplegia, it clearly depends upon the pressure on one side of the pons Varolii by the blood effused. It is quite evident that it is not a loss of function of the parts altered in the cerebellum which is the cause of the paralysis, as a great many cases of alteration of the same parts have been observed without the least appearance of hemiplegia. Neither is it an irritation of the cells or nerve-fibres of the cerebellum which causes a hemiplegia, as when such an irritation causes a paralysis it is in the limbs on the side of the alteration, while in cases of hæmorrhage in one half of the cerebellum the paralysis exists in the opposite half of the body. A pressure on one half of the pons Varolii explains very clearly the production of a hemiplegia in the opposite side of the body; and the existence of a pressure on that organ cannot be doubted when we remark that even more distant organs (the cerebral lobes) are often then submitted to pressure, as shown by the production of a comatose state. Indeed the difficulty, instead of being to admit that there is a hemiplegia owing to pressure on the pons Varolii, would rather be to explain how there is not always a hemiplegia caused by that pressure in cases of hæmorrhage in one of the cerebellar lobes. It is, however, easy to understand that the pressure, except when the hæmorrhage takes place very near one side of the pons Varolii, is distributed over the whole of that organ, and that it therefore causes a general weakness instead of a localized paralysis.

4. *Anæsthesia*.—What has been said of hemiplegia applies also to anæsthesia in one half of the body; and if this symptom is so rare, it is in consequence of the fact that more pressure upon a nervous centre as well as upon a nerve is required to produce anæsthesia than to produce paralysis.

5. *Hyperæsthesia*.—The mode of production of hyperæsthesia, whether after a lesion of the cerebellum or after a lesion of the posterior columns of the spinal cord, is yet involved in great obscurity. Only one thing appears to be clear in respect to it, and it is that the increased sensibility depends upon a change in other parts than those injured, and not upon a change in the injured parts themselves.

6. *Amaurosis*.—In cases of hæmorrhage in the cerebellum, this symptom is much less frequent than in cases of inflammatory softening, abscess, or tumour of the cerebellum; and it seems extremely probable that in the few cases of cerebellar hæmorrhage in which amaurosis has been observed, there was an inflammatory softening of the cerebellar tissue round the clot. Amaurosis is a result of an irritation of certain parts of the cerebellum acting upon the nutri-

tion of some parts of the nervous apparatus of vision. It is not usually on account of a pressure upon the tubercula quadrigemina that amaurosis exists in cases of disease of the cerebellum, as we find that loss of sight is sometimes observed in one eye only, and that is the eye on the side where exists the alteration in the cerebellum; while, if it were owing to a pressure on the tubercula quadrigemina, the loss of sight would be in the eye on the opposite side.

7. *Deafness and Tinnitus Aurium*.—These symptoms are not due to a loss of function, or to an irritation of some fibres of the auditory nerves existing in the cerebellum, as we find, in most cases of hæmorrhage or other alterations of the cerebellum, no paralysis of the auditory nerves, and no tinnitus. These symptoms may sometimes be caused by pressure upon the medulla oblongata, and tinnitus may be due also to a simple congestion.

8. *Convulsions*.—Mechanical injuries to the cerebellum (such as pricking, cutting, &c.) in animals, do not produce the least convulsive movement in any muscle of the trunk, face, and limbs. But it is probable that new properties may be acquired in consequence of an inflammation around the clot some time after a hæmorrhage in the cerebellum, just as is the case in the brain, or in the grey matter of the spinal cord; and then convulsions may be caused through the transmission of the irritation to some other parts of the encephalon. But in ordinary cases, when convulsions take place at the time the hæmorrhage occurs, they are due to a pressure upon the pons Varolii, and sometimes also on the medulla oblongata.

9. *Vomiting*.—This symptom is, perhaps, the only one which seems clearly to result from an irritation of the tissue of the cerebellum when it is not altered by inflammation. It is one of the principal symptoms of all the diseases of the cerebellum.

10. *Breathing*.—The difficulty of breathing in cases of hæmorrhage in the cerebellum is very easily explained by the pressure then existing upon the pons Varolii and the medulla oblongata.

When we look upon the three sources of symptoms in cases of hæmorrhage in the cerebellum—viz., *pressure, irritation, loss of function*, we find that most of the symptoms are entirely or partly produced by the pressure of the effused blood upon other parts of the encephalon, and not as a consequence of irritation or loss of function of the cerebellum. Hemiplegia and anæsthesia, as well as coma, difficulty of breathing, &c., are of that class. The symptoms of irritation are to be divided into two groups—one containing those symptoms which may occur by the irritation of the tissue of the cerebellum while it is still in its normal condition, the other containing the symptoms of irritation of the cerebellum when the vital properties of its tissue have been modified by inflammation. In the first group are but two symptoms—vomiting and, *perhaps*, amaurosis. In the second group are hyperæsthesia and the symptoms of the first group—i.e., vomiting and amaurosis. No evident symptom can be considered as due to a *loss of function*, except a general weakness.

Dr. Brown-Séquard does not speak of headache as a symptom of hæmorrhage in the cerebellum, because this symptom is variable,

and exists so frequently in cases of disease of any part of the brain, or without any such disease, that it has no diagnostic value. However, a violent and persistent pain in the back of the head, and co-existing with symptoms exciting a suspicion of hæmorrhage in the cerebellum, would give an additional value to these symptoms.

ART. 34.—*An Account of an Epidemic of Hysterical Demonomania.*

By Dr. CONSTANS.

(*Annales Méd.-Psychologiques*, and *Psychological Journal*, January, 1863.)

In the ancient province of Chablais, now in the arrondissement of Thonon (Haute-Savoie), is a commune of 2000 inhabitants, named Morzines, situated at the extremity of the valley of Aulph. It is separated from Valais by a mountain only; its altitude is about 1500 metres, the climate severe, and the vegetation tardy. The people are very poor, and occupy the basement story of wretched huts; they live in a half-asphyxiated state, huddled near cast-iron stoves, heated even to redness. Their food consists of barley bread, potatoes, and smoked meats; and for drink, water, always very cold, is alone used. Their aspect, as a rule, is pitiful; the lymphatic-nervous temperament predominates; childhood is sickly and not easily passed; the fecundity of families is very great; adult age is prematurely decrepit, and old age rare. In other respects, these people are gentle, honest, obstinate, very devout, and excessively superstitious; little intelligent at the best, their judgment is further obscured by a multitude of absurd beliefs.

In March, 1857, two little girls, blondes, very pious, puny, but withal healthy, became the subjects of certain extraordinary attacks. Presently the affection extended to others, and in seven months twenty-seven persons, children, young girls, and women, were seized by it. It was averred that during the paroxysms of the affection, the children spoke French with surprising facility, or responded in German or Latin, lost all family affection, became surprisingly insolent and impious, and exhibited extraordinary physical powers, four men being insufficient to restrain a single child. Moreover, the children would climb, in the twinkling of an eye, to the tops of trees, and then turn somersaults, or leap from one tree to another, removed many metres, and descend to the ground head downwards.

Towards the end of 1860, the number of "possessed" (for as such they were regarded in the commune) had increased to 110; and in April, 1861, the Minister of the Interior directed Dr. Constans, inspector-general of lunatics, to visit Morzines, and make inquiry into the nature and causes of the outbreak, and to take such steps as might be needed to put an end to it. When Dr. Constans reached Morzines he found the population much depressed, every one going in fear of devils, and a deep irritation reigned against the sorcerers, the authors of the evil. The treatment of the affected, up to this period, had consisted in parental intimidation, exorcisms, pilgrimages,

and magnetism. Seventeen of the twenty-seven persons attacked in the first seven months of the epidemic had been cured, it was stated, by exorcism. Certain children had recovered spontaneously, and in others the attacks had yielded to promises, or threats of death.

Dr. Constans examined sixty-four of the "possessed." They were mostly celibates, hysterical, chloro-anæmic or scrofulous, and suffering from gastralgia, amenorrhœa, or dysmenorrhœa; the appetite was capricious, the sleep inconstant and light. Idle, loquacious, exalted, and fantastic, they flocked together, card-playing, exciting themselves mutually, and masking the insufficiency of their aliment by the immoderate use of black coffee. Everything gave occasion for a paroxysm, but nothing produced one so surely as the expression of a doubt that they were "possessed." The paroxysm was ushered in by yawnings, pendiculations, startings, choreiform jerks, alternations of dilatation and contraction of the pupil, and a frightened aspect. Cries, vociferations, and oaths supervened. The physiognomy became dejected and assumed an expression of frenzy. The respiration was panting, and the movements, at first confined to the superior parts, extended successively to the trunk and extremities. Aggression commenced; furniture, chairs, or stools were cast at the spectators; then the convulsionaires precipitated themselves upon their parents or upon strangers, struck them and struck themselves, bruising the chest or body, whirled about now in one direction, then another, and cast themselves on the back, starting up again as if it were on the rebound of a spring. No erotism mingled with the idea of demoniacal possession, and the affected never uttered obscene words or were guilty of lubrical actions. In the most disordered actions they never exposed the person. The paroxysm endured from twenty to twenty-five minutes, the pulse becoming enfeebled or lost; but the beating of the heart remained normal, while the hands were icy and the feet cold. Towards the decline of the paroxysm, the noise became less, the movements diminished in rapidity, there were eructations, the affected looked around with astonishment, arranged their hair, replaced their caps, drank several mouthfuls of water, and recommenced work, declaring that they felt no lassitude and remembered nothing. It was evident, however, that the first assertion was not altogether true, and they heard and saw perfectly during the attacks; closing the eyes if menaced with a blow in the face, and avoiding, under all circumstances, places or bodies which might injure them when they cast themselves upon the ground.

Dr. Constans looks upon the outbreak as of an hysterical character, and upon the affected as not altogether responsible for their acts. To this conclusion Dr. Legrand du Saulle (from whose abstract of Dr. Constans' account we derive the foregoing facts) demurs. Milder means proving unavailing, Dr. Constans quickly brought the epidemic to an end by having the priest of Morzines removed, and requiring the commune to be occupied by a brigade of gendarmerie and a detachment of infantry. The people were intimidated, and the "possessions" ceased.

ART. 35.—*A Case of Traumatic Tetanus treated by Nicotine.*

By Mr. TUFFNELL, Surgeon to the City of Dublin Hospital.

(Dublin Medical Press, January 7, 1863.)

CASE.—J. D., aged 42, a tall, muscular man, a sewer-maker, of very intemperate habits, was brought to the City of Dublin Hospital on the evening of the 28th of February, 1862, suffering from a severe compound fracture of the right radius, three inches above the wrist. The accident had occurred about twenty minutes before admission, and was occasioned by the falling of a heavy flagstone upon the forearm. There was a good deal of laceration of the soft parts, and considerable hæmorrhage. The wound, which was three inches long and triangular in shape, had its base to the ulna, and was filled with blood and clay; the upper end of the lower fragment of the radius was thrown outwards upon the ulna; the hand was pronated and curved inwards.

The wound having been carefully cleansed, the fracture was adjusted, and the arm placed upon a pistol splint; water-dressing with flannel-tailed bandage surrounding all. Wine and an opiate were given. Everything went on well until the evening of the 12th of March, when the patient said that he thought he must have caught cold, as he had pain and soreness about the jaws, with difficulty of mastication. He had also a peculiar tendency to sigh. Shortly afterwards he complained of pain in the left mammary region, and the abdominal muscles, when examined, were found somewhat tense. There was at this time no rigidity of the forehead, eyelids, or mouth. The pulse was soft, regular, and natural at 68, the respiration normal, and the bowels had been acting regularly. He was ordered, however, a bolus of five grains of calomel with one drachm of compound powder of jalap.

On the following day (13th), the facial muscles exhibited the characteristic contractions of tetanus. The trismus was now determined, and the cervical muscles, especially upon the right side (that of the accident), engaged. The right rectus abdominis was more tense than the left. The bowels had been freely acted on by the aperient, and the discharges were not dark.

He was now ordered to take one drachm of chloroform agitated with an ounce and a half of linseed-tea every fourth hour, and a bag of ice directed to be applied to the cervical spine. The wound was looking healthy, and beginning to cicatrize at the edges.

Upon the 14th there was an advance in the symptoms, which seemed to be kept partially at bay only by the chloroform. The muscles of the neck and abdomen had become exceedingly tense, and there was pain through the attachments of the diaphragm. The muscles of the extremities, too, were now attacked by clonic spasms. The pulse had increased. Chloroform was continued.

Upon the 15th he was worse. The masseter muscles and sterno-mastoids were very rigid, and the respiration was rendered difficult by the viscid mucus which accumulated in the bronchi, and could not be got rid of by expectoration. (He had previously suffered frequently from bronchitis.) He could swallow liquids, but with great difficulty. As the bowels had not acted since the 13th, a purgative enema was ordered, and the chloroform directed to be given more frequently, every third or second hour, as required.

On the 16th the symptoms were unaltered, but the pulse was becoming frequent, rising to 90 during the paroxysms of pain. The chloroform having been administered for three days, and the case not beginning to improve, it

was determined to employ nicotine, and the Rev. Professor Haughton having kindly brought some of that prepared under his own direction, the strength of a single drop of which is equivalent to 23·2-10th grains of Virginian Cavendish tobacco, the first dose was administered at eight P.M.; one drop being dissolved in a dessert-spoonful of white wine mixed with an equal quantity of water. The pulse, prior to the nicotine being given, was 72; it speedily rose to 84, and within *forty seconds* of the medicine being swallowed, the whole surface of the body was bedewed with sweat. The welling up of the perspirable fluid through the pores of the skin was remarkable, it being absolutely perceptible; these drops of fluid being so large that they might easily be flicked with the hand from off the forehead. The spasms almost immediately relaxed, the corrugation of the brow and frightened expression of countenance disappearing. This improvement was, however, only transient, and the nicotine was repeated every two hours up to two A.M., when, in consequence of the unpleasant taste and nauseating effects, the patient refused to take any more. At six A.M. he consented, however, and continued to do so regularly every two hours, the dose being increased to two drops, or nearly a drachm of Cavendish tobacco. The stools brought away by the turpentine injection were for the first time dark green and fetid. From the commencement of the attack a most liberal allowance of nourishment, in the shape of essence of beef, jelly, and wine, had been allowed, which was still continued, although swallowed with great difficulty, and got down between the wedges which separated the molar teeth. Fomentations of a strong decoction of tobacco were applied over the abdomen, and with apparently some good effect; a liniment of croton oil was also rubbed over the surface; and the wounded arm enveloped in a soft anodyne linseed poultice, made with a drachm of the extract of belladonna dissolved in a pint of boiling water.

18th.—Seven days had passed over since the first symptoms had exhibited themselves, and two since the nicotine had been employed. All matters were now aggravated. The patient had become unmanageable, and would take no medicine by the mouth, and fluid of any kind he could hardly swallow. The spasms were increased in intensity, of opisthotonic character, and recurring at intervals of eight minutes. The breathing was very quick and laboured, being 44 per minute, and the face congested from mucus, which was kept churning up and down in the trachea; the pulse 130 in the minute; the surface of the body wet and cold. As it was impossible to give the medicine by the mouth, a couple of drops of nicotine were added to a glass of wine-and-water and injected into the rectum and held. The spasms relaxed under its influence, the power of expectorating returned, and the heart's action became regular again. Towards the afternoon delirium set in, and the patient now acquired full voluntary power over the muscles of the arms and legs. He endeavoured to get out of bed, and struck wildly at those around him, and was so violent that it was necessary to put him under the influence of chloroform by inhalation, which was soon accomplished, to the great relief of the patient, who afterwards rested quietly for some time. He was conscious on awaking, and asked for water to be given him to drink. He was, however, so much exhausted that two ounces of whisky, with an equal quantity of warm water, were thrown up the rectum and retained, after which he took some strong beef-tea by the mouth. During the violence of the patient, the fractured radius became bent almost to a right angle with the ulna, and the deformity of the arm was consequently extreme. An attempt was now made to remedy this deformity, but such aggravation of the symptoms was immediately produced that it was necessary to discontinue the effort, and the arm allowed to assume the position the

patient chose, a scruple of watery extract of opium dissolved in four ounces of water being first applied (by means of lint saturated with this lotion) to the wound. The bowels being now confined, an enema of turpentine, assa-fœtida, and castor oil was ordered to be administered, which secured the discharge of some dark green foetid stools.

19th.—The nicotine in two-drop doses was now injected by the rectum regularly every second hour, the patient being supported by wine, beef-tea, and essence of meat, introduced through the opening left between the wedges placed between the molar teeth. Tobacco fomentations were applied to the abdomen of the strength of a drachm to the pint of boiling water.

20th.—Patient complained to-day of great pain in the biceps muscle of the affected arm, and also under the scapula of that side. Suffocative catarrh was also present. A mustard plaster enveloping the whole chest was ordered to be applied, and the whisky enemata repeated, the nicotine being continued in injection every two hours. The discharge from the wound was now purely sanguineous, and all attempt at cicatrization had for some while ceased. The retraction of the anus was now so great as to render the introduction of the enema-tube an act of much difficulty.

22nd.—The fifty-sixth drop and the last dose of nicotine administered to-day, the countenance being placid, and no spasm having occurred for some hours. There was great aching pain in the lumbar region, and soreness of the muscles generally, especially on the right side of the body. The urine passed to-day contained a highly lateritious deposit, but did not possess the smell of nicotine. Patient very weak. He was now ordered an injection consisting of five grains of quinine, the yolk of an egg, an ounce of whisky, and three ounces of milk, to be administered every sixth hour.

23rd.—The power to open the mouth to some slight extent had returned. The discharge from the wound showed signs of purulent secretion.

24th.—Patient not so well; pulse varying in force and rapidity from 104 to 140; he had pain in the diaphragm and occasional spasms; the discharges from the bowels brought away by an oil draught given overnight were black and offensive. The aperient was repeated by the mouth with a full dose of opium shortly after.

26th.—Better, though very weak.

28th.—Slept well; was free from pain and spasm, and the wound looking florid and healing in. The quinine injection repeated night and morning.

31st.—Considerable soreness of the abdomen and diaphragmatic region being complained of, a large belladonna plaster was directed to be applied over it.

From this date the recovery was gradual and progressive, and he was discharged from hospital in good general health, and with partial use of the injured arm, on the 10th of May.

I have abbreviated this case (as far as I well could) so as not to omit the leading features and facts. That the man owes his life mainly to the nicotine, I think must be fairly assumed, for under the use of chloroform he was gradually getting worse, and the spasms not coming under control. It is true that for a considerable period after commencing the nicotine treatment the disease progressed in intensity; but still a decided impression and relaxation of spasm followed upon each separate employment of the remedy. To chloroform, however, I was greatly indebted for the power which it gave me in controlling the violence of the delirium, and obtaining for the patient and his attendants rest.

ART. 36.—*Case of Tetanus from Strychnia Poisoning treated by Chloroform Inhalations.*

By Mr. J. BLACK, President of the Medical Society of Victoria.

(*Australian Medical Journal*, October, 1862.)

CASE.—“One evening, about a month ago,” writes Mr. Black, “I was urgently requested to see a man who was supposed to have taken poison: I found him suffering from decided symptoms of strychnine poison: he was stiff about the jaws, and was grinding his teeth fearfully. As soon as he was able to answer my question, he confessed that he had taken as much of the poison as would lie on a sixpence. I sent a messenger to the nearest apothecary for 3ss. zinci sulph., which I directed should be administered while I ran to my house for chloroform. I returned in about twenty minutes. The emetic had been given nearly a quarter of an hour, but had not acted. The symptoms had increased in severity, and on attempting to administer chloroform, he had a spasm which almost killed him. I however succeeded in producing complete anæsthesia; on recovering from which, the emetic began to act, and he vomited freely the contents of the stomach. On recurrence of the spasms, I had again recourse to chloroform during, perhaps, a couple of hours, when the urgent symptoms abated. The patient then commenced sweating profusely, and was very prostrated: I therefore ordered brandy and water. In visiting him next day, I found him quite free from spasms; but he did not recover his usual strength for several days: he was a young man, not thirty years of age, muscular, and well formed.”

ART. 37.—*A Case of Traumatic Tetanus cured by Alcoholic Intoxication.*

By Messrs. COLLIS and WILMOT.

(*Gazette Hebdomadaire de Médecine et Chirurgie*, January 2, 1863.)

This case is quoted from the *Dublin Medical Press*, but without giving number or date. We translate all the particulars furnished by our Parisian contemporary.

CASE.—A boy, aged 9, injured his arm seriously by a fall from a carriage of some sort. Amputation was thought necessary, but not submitted to. On the eighth day general tetanus set in, with trismus. The treatment pursued was *punch*, half and half, and in quantity sufficient to induce complete drunkenness. After seven or eight glasses the contractions and all the “accidents tétaniques” diminished; and at the end of three or four days they had completely disappeared. The child recovered perfectly.

ART. 38.—*Case in which Tetanus and Paralysis co-existed.*

By Dr. HUNT.

(*American Quarterly Journal of Medical Science*, January, 1863.)

CASE.—R. C., a boy, aged 16, was admitted into the Episcopal Hospital on the 12th of April, 1862. He had been injured in a rope factory by a

machine, which I personally inspected afterwards. It is a large wheel, some twelve feet in diameter, and eighteen inches broad at the circumference. The outer surface is studded very thickly with polished iron spikes, three and a half inches long, very sharp at the points, and about one-fourth of an inch thick at the base. In fact, it is an immense circular comb, revolved by steam, and used for the purpose of disentangling and layering hemp.

While revolving, the machine requires a man, and an adept, to attend it; but in this case the proper personage was temporarily absent, and the boy, full of ambition, undertook the duty. He was caught, and after being torn by the points of the spikes, was fairly impaled by three of them entering his cranium. Luckily, some one near threw off the belt, and the wheel stopped; but so firmly was the boy fixed, that it took two men, one to support the body and the other to insert his fingers between the spikes, to draw him off. The person who acted in the latter capacity told me that the boy was perfectly conscious, did not think he was much hurt, and wished to walk home or to the hospital. His condition on admission was as follows: There were some eight or ten large lacerated wounds of the back of the *right* hand and arm, the integument being thrown off in flaps, exposing the tendons, muscles, and superficial vessels and nerves. These wounds reached as high as the elbow, and the distances between them corresponded with the rows of spikes. There was no fracture of the bones of the arm or laceration of the muscles. The next point of injury was the left malar bone, which was fractured by one of the spikes, and then the frontal and parietal bones appear to have been pierced. There were three punctured fractures of the *left* side of the cranium, two through the frontal bone and one through the parietal. One of the former was at the frontal protuberance; the other was an inch and a quarter from the first, and just within the temporal ridge. The puncture of the parietal bone was on a line half way between the other two, and a little more than an inch posterior to the coronal suture. We thus have a tripod of spikes indicated, upon which the boy was impaled.

There were no brain symptoms whatever at the time of admission. The reflected flaps of integument on the arm were returned and secured by sutures, and water dressings were applied to the head and face. This was on a Saturday. The boy did perfectly well until Monday, when there were intervals of delirium; but most of the time he was rational, although quiet and indisposed to talk. He continued thus until Wednesday, when hemiplegia of the *right* side came on. On Thursday the patient was reported to me as very much worse. I found him unable to speak; when he made the attempt, he did nothing but mutter. He was, however, conscious, when aroused, his eye having an intelligent expression, not according with his inability to speak. On trying to open the mouth to protrude the tongue, I found that he could not use his jaws properly, and was not able to separate them more than half an inch. The inability was greater upon the *left* side than upon the right, the paralysis of which continued unchanged.

So preoccupied was my mind with the idea of compression, that tetanus did not immediately occur to me. A consultation was ordered at four o'clock Thursday afternoon. On my way home, in thinking of the anomalies of the case, the question arose, "Has this boy tetanus and paralysis together, and is one condition influencing the other?" Drs. Kenderdine and R. P. Thomas met in consultation at the time proposed, when my suspicions were fully confirmed. Well-marked trismus, the very characteristic risus sardonicus, and hard abdominal muscles, showed unmistakably the invasion of tetanus. At the same time the paralysis of the right side continued, and although perfect as to the arm, the patient once in a while moved the right leg, but apparently had no voluntary power over it. The rectum and bladder were also paralysed, and from

this time until the termination of the case, the fæces and urine were discharged involuntarily. It was concluded not to trephine. The sutures were removed from the arm wounds, and warm water dressings applied. The patient was also given chloroform gtt. xx, fluid ext. of conium gtt. x, every two hours. Under this treatment he became much more quiet. On Friday the tetanus was most thoroughly confirmed, by a remarkable combination of empros and pleurosthotonos, that is, while there was a forward bending, the body at the same time was arched towards the left, as though the paralysed side had no power of resistance. Throughout Saturday the symptoms continued unchanged, but on Sunday the boy became entirely unconscious, and died on Monday morning, on the tenth day from the injury.

Post-mortem.—The three punctured fractures of the cranium—at the points before indicated—were as clean on the external surface of the bone as though made by a sharp-cutting punch. No fissures radiated from them. Small rough fragments of the internal table projected inwards from the margins of the holes, at which they maintained their connexion with the sound bone. One of these was at least a half inch in length, and was connected with the fracture of the frontal protuberance. The membranes and brain were lacerated at points corresponding with the fractures. The right anterior lobe was the seat of a large abscess, and this lobe was more lacerated than the middle one. There was no particular congestion or inflammation of the base and medulla. The spinal cord was not examined. The wounds of the arm appeared to be in good condition. An abscess had formed about the left knee-joint. The other parts of the body were perfectly normal.

ART. 39.—*On the Aura as a Guide to Treatment in Epilepsy.*

By Dr. RAMSKILL.

(*Medical Times and Gazette*, August 30, 1862.)

In a clinical lecture on epilepsy, delivered recently, Dr. Ramskill says:—"In conclusion, I would say to you as a fact of the greatest importance, when you have a case of epilepsy, endeavour to find the aura. If it exist in the extremities it may be accompanied by a sensation of pricking, tingling, or indefinite sensation, or by cramp. If you can localize it, try what blistering or the ligature will do, or after blistering apply to the denuded surface chloroform liniments; if these fail, try the actual cautery, and if in a limited locality, as in a spot, the moxa: lastly, section of nerves going to the part; if there be signs of an old wound, open the seat of it, destroy the cicatrix before trying section of the nerves. If the patient insists he has no premonitory warning, and the fits come at regular intervals, ligature each limb successively; failing in this, try galvanism over the whole surface of the body, let the power be strong and the conductors dry. Your object is not to induce muscular contraction, but a commotion in the excito-motory system of superficial nerves. There is a girl now in the hospital in whom galvanism will produce a fit when one pole passes over the region of the left ovary. In this case I believe ovarian irritation is the cause of the fits; the galvanism (with a dry conductor) does not penetrate dynamically to the ovary, it acts in a different manner, which I will explain at another time."

ART. 40.—*On the Treatment of Epilepsy by Galium Album.*

By M. GARNIER.

(Lancet, June 21, 1862.)

A witty writer in *L'Union Médicale*, M. Garnier, furnishes very interesting data respecting the amount of confidence which should be given to the establishment near Tain, where epilepsy is alleged to be cured by the use of the 'galium album.' From the details given by the author, it appears that a great deal of trickery is practised, and a good deal of money is made by the sale of the lozenges. Nothing, moreover, is more doubtful than the cures; and the value of the remedy may be judged by the fact that it was *officially* tried at the Bicêtre, near Paris, upon eight picked cases, and signally failed.

ART. 41.—*On the Therapeutical Value of Cod-liver Oil in Chronic Convulsive Diseases.*

By Dr. ANSTIE, Assistant-Physician to the Westminster Hospital.

(Medical Times and Gazette, March 28, 1863.)

The convulsive diseases in which the author has employed cod-liver oil are paralysis agitans, simple epilepsy, chorea, and mercurial tremor, and in all these affections it has appeared to be more constantly useful than any other medicine. Of paralysis agitans, four cases are detailed, of which three were very decidedly improved, and one of them may have been said to be cured, although the affection was very severe. Of chorea, one case is detailed, and others are alluded to in which the benefit produced was very marked. Of mercurial tremor, one most remarkable case is related, in which the cause of the mischief was a very unnecessary salivation inflicted by medical authority some thirty years previously; the patient was attacked immediately afterwards with dreadful tearing pain in the muscles of the fore-arms and calves, and with violent muscular tremors, and ever since that time she has been liable to a recurrence of the symptoms when much fatigued or depressed from any cause. On application to Dr. Anstie, at the Chelsea Dispensary, cod-liver oil was prescribed and persisted in for five weeks, at the end of which time all the symptoms had perfectly disappeared, the patient declared that she had never been cured before in less than six or eight months, and she doubted whether any other medicine than the oil had ever really done her any good. Twice since she has had slight recurrence of the symptoms, but a short course of cod-liver oil has on each occasion given complete relief. Of simple epilepsy, twenty cases are given, in which the treatment had been confined to the use of cod-liver oil. Of these there were five upon whom no good effect whatever was produced; seven had completely recovered; two had disappeared from supervision at a time when they were

rapidly improving, although they could not be said to be cured; in two others the mental symptoms had greatly improved, but the fits remained as before. Four patients still remain under supervision; in two fits have ceased, although there are still frequent prodromata; and in the remaining two but little good has yet been effected. Besides this general summary of results, Dr. Anstie details the particulars of three cases which from their severity might be said fairly to test the remedial power of the oil. The patients were respectively a girl, aged 17, a boy, aged 13, and an infant, aged 7 months; in all of them the fits were very frequent and severe, and the nervous system exhibited signs of great depression. The case of the infant is specially noticeable, because it is proved by microscopic inspection that the milk of the mother was very deficient in oily matter, and it appeared that in a former infant of the same mother precisely the same train of symptoms had appeared, and had terminated fatally. In all these three cases the treatment has proved perfectly successful, and the author comments strongly on the fact that in all these cases the general nutrition of the body had been excellent, and only that of the nervous system had appeared deficient, and said the conclusion appeared inevitable that the oil had expended itself in enriching the nervous centres. This, indeed, was the principal point of the paper. The author directs attention to Dr. Radcliffe's remarks on the necessity of fat to the nutrition of the nervous centres, and mentions the fact that that gentleman has found cod-liver oil of the highest value in the treatment of convulsive diseases. He observed, also, that the beneficial action of cod-liver oil was quite consistent with what we know of the action of the few other remedies which careful therapeutical investigation has credited with a really beneficial action in chronic convulsive diseases. Steel, arsenic, quinine, all these may fairly be spoken of as foods. With regard to sedatives, the author remarked that in the first place the good effects which could be expected from them were chiefly temporary, and such as result from breaking through for a time the evil habit, so to speak, of convulsive action. Secondly, and this was most important, there was strong reason to believe that it is not the really narcotic effect of these remedies which is of service in preventing or arresting convulsive action, but merely the stimulant effects which can be obtained from small doses; for there is no class of remedies which is more useful in preventing or arresting convulsions than the pure stimulants. The author concludes his paper by deprecating strongly any return to the absurd system of hunting about blindly for "specifics" for chronic convulsive diseases. The progress of clinical observation is blasting the reputation of one after another of the strange, out-of-the-way remedies which had once been accepted with the blindest faith, and is pointing unmistakeably to a rational treatment of convulsive diseases by means of medicines whose action it is possible to understand.

ART. 42.—*A Model Hysteric.*

By Mr. ROBERT ELLIS, Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

(*Medical Times and Gazette*, April 4, 1863.)

CASE.—Miss A. B., an unmarried lady, in easy circumstances, in the winter of 1829 first became the victim of a disorder which, true to its protean character, assuming many aspects, never wholly left her for thirty years subsequently. She was at that time an excitable and impressible girl of eighteen; but I have been unable to discover any distinct emotional cause for the development of her hysteric disposition. She had a cough and pain in the right side, which was treated in the manner customary at that period—by the repeated application of leeches. In the following spring she was taken to Dr. Abercrombie, who advised leeches again, followed by a blister, and this was repeated for several months, the purely hysteric element in her case never having been recognised. In 1831 she became a patient of the late Sir H. Marsh, who, recognising the real type of her disorder, prescribed, with a temporary relief, shower-baths and fresh air. But, her pain becoming very severe as winter approached, she again returned to Dublin, and this time was cupped on the side. From that moment a new symptom made its appearance in complete paralysis of the bladder. This was at first overcome by injections of turpentine and castor oil and warm hip-baths. But in a short time these remedies lost all influence, and she was allowed to remain with the bladder unrelieved for thirty and forty hours. Sir Philip Crampton, who then saw her, and was apparently perplexed by her state, directed the catheter to be used twice daily. This, however, aggravated her distress from the excessive irritability of the urethra, to relieve which large doses of laudanum were administered. She was now blistered on each side of the spine, and occasionally over the pubes, with but little good result. Issues were opened at intervals down the spine, and kept discharging for a considerable time. Thus the unhappy sufferer dragged on a painful life until 1834, when the hysteric cough came back, and was again attacked by repeated blisters. She had now fully learned the soothing influence of morphia, and was supplied with as much as she required. The disorder again returned to the bladder, and was this time overcome in a rational manner by the use of enemata of starch and opium into the rectum. So effectual was this remedy, that for many months the disorder did not recur; but the cough returned with its disappearance, and probably would have continued but for an intercurrent attack of diarrhoea, which left her quite free from all hysteric symptoms for several months.

In 1836 great domestic sorrow and anxiety prepared the way for a return of the old disorders. The bladder became as helpless as ever, and the old treatment of escharotics and issues to the spine was had recourse to. The hideous seams and numerous white cicatrices still visible on the back of this unhappy lady would almost suggest her having passed through some sanguinary field of battle. It was perhaps unfortunate for her that this violent treatment afforded a temporary relief, since it was persisted in to a most formidable degree. Suddenly the bladder acted as well as if nothing had happened, and now the patient got into a fresh trouble. She was taking pills with a minute quantity of strychnia. "I do not know whether the pills did me good or harm, but about this time I got a trick of falling down when I attempted to walk, and after a laudanum injection; but by keeping quite quiet came all right again." The hysteric paralysis had seized the

voluntary muscles this time. Headaches and confusion of intellect now occurred, and offered a new and tempting opportunity for vigorous treatment. She was dry cupped again and again on the nape of the neck, and as soon as the glasses were removed a seton was inserted. Her dose of laudanum now reached two drachms.

In this state she became a patient of an eminent physician at Leamington. There was now, instead of dysuria, a constant flow of pale urine. Her treatment commenced with calomel in cold gruel, and the laudanum was stopped. She became much reduced, and all her distresses were aggravated, especially at the menstrual periods. A tonic treatment was attended with a better result, and in a few months she became for a time tolerably well. Occasionally, however, the palsy of the bladder would recur, and was relieved by a dose of Jeremie's solution of opium. Then diarrhoea seemed to take its place, and I think there can be little doubt that this symptom had a purely hysteric origin. The old seductive influence of the opiates became again felt, and they were freely used.

In May, 1843, the bladder again giving trouble, suddenly, and without any previous warning, a severe attack of trismus ensued. The first invasion of this new malady was short, lasting only an hour or two at a time, and *invariably induced by irritation of the bladder*. Soon, however, it fully established its claim to a place in the chain of her morbid tendencies. The jaws would continue closed for a day, then for days, and at last even for months, with intervals of respite. In one of these the eyes suddenly closed and remained shut for a whole month! resisting all treatment and efforts to open them. During this period, the hysteric irritability being apparently at its height, she lost the use of every limb, and was perfectly helpless. The shock of being roughly carried downstairs and into a new house broke this spell, and in a tempest of hysterical passion she opened her eyes, and they only occasionally closed again for short periods. The trismus, however, would not let her go, and one long fit lasted for nine months, terminating, as usual, suddenly. She was taking much opium all this time. Sometimes she appeared comatose for eighteen hours, and then woke with an open mouth. If the trismus had lasted for days it was always at an end immediately on the occurrence of a fit of hysterics. Sometimes it yielded (as, indeed, might have been expected) on the sudden application of scalding water to the cheek.

Down to 1850 the state of this lady remained with little alteration, the trismus and the urinary difficulty only occasionally abating, and never wholly leaving her. In that year, during a period of home trial, the trismus relaxed, and was replaced by a violent spasm of the muscles on one side of the face, distorting the countenance in the most horrible manner, and entirely preventing her swallowing even liquids. The inhalation of chloroform was then commenced, and proved effectual in subduing this spasm for a time. This new malady held its ground for a long time, and the chloroform became a necessity in order to enable her to take her meals. An attempt was then made to bring her under the influence of mercury, and she had the advantage of a scruple of calomel for a dose once a week! but no salivation ensued—in all probability the bowels and kidneys carrying out the drug. Prolonged salivation was, however, at last the reward of a persevering inunction of mercurial ointment, with smaller doses of calomel often repeated. The spasm and trismus then vanished for two months.

An investigation into the state of the uterus was now made, and it was discovered that a small polypoid growth was hanging from the cervix. This was twisted off, but no change in her state ensued. She became rather worse, and used up at least a pound of chloroform every three weeks! Thus the case continued down to 1855, to the dismay of all who attended

her. In the spring of that year a mimic paralysis agitans appeared on the scene, occurring with marked violence at the return of each menstrual period. In this state of hysteric oscillation, from the semblance of one disease to that of another, she came under my care. This was in the autumn of 1858.

The chloroform bottle, the opiate, and the catheter were now indispensable to her. She was sallow, haggard in expression, nervous and emotional in the highest degree. She still menstruated with great regularity, and her trismus and inability to empty the bladder remained as bad as ever. The teeth of the upper jaw, from constant pressure on the corresponding teeth below, were worn down in a remarkable degree, and the masseter muscles on each side had undergone a degree of hypertrophy very significant of their constant exertion during so long a period. A careful examination into her case removed from it, with two exceptions, every trace of organic disease. The uterus was unhealthy. There was much congestion and some enlargement of the cervix, and from its posterior lip hung down a small polypus, half the length of the little finger. There was in addition an unhealthy bunch of inflamed piles. After removing the polypus, and by repeated applications of nitrate of silver, and once or twice of stronger escharotics, the cervix was got into a perfectly healthy state. During this time a great amelioration took place in her state of general health. We had agreed to put the chloroform and the opiate on the shelf, and the catheter was also locked up. I then removed the piles, some by excision and some by ligature; "and from this," writes the patient, "I rose up in about ten days more free from pain than I had been for years." She then went into the country, and in a few months gained so much in strength and flesh as to be a wonder to herself and others—chloroform, opium, and the catheter were all disused and nearly forgotten.

It was certainly a triumph over the hysteria, but, as it proved, it was not of very long duration. The menstrual crisis was now approaching, and we were to have one more long struggle with this obstinate, often beaten, but unconquered foe. The monthly discharge beginning to occur irregularly, was at last attended on one occasion with much pain and discomfort and a feeling of bearing down, in fact, a state of physiological congestion without relief by the usual flux. The old irritation appeared to leap back into its accustomed seat, and in April, 1859, this poor lady presented herself again as bad as ever, and, I am sorry to add, the opium had resumed its accustomed place on her dressing-table. I once or twice applied two or three leeches to relieve the uterine congestion, and always with benefit to her other symptoms; but as it was obvious this could not be repeated constantly, it became desirable, if possible, to overcome the hysteric disorder in some other way.

As there appeared to be some irritation in the canal of the urethra, relief was given to that by smearing the catheter with a little extract of belladonna. Hypodermic injections of morphia and other anodyne substances into the arm were then used, and invariably opened the mouth in a few minutes; occasionally also chloroform was given. In fact, the old routine was being gradually reopened, during the time I was awaiting the completion of one of Legendre and Morin's instruments for applying the electricity of induction, which Duchesne has called after the name of its discoverer, Faraday. Upon the application of this remedial agent (the only means as yet untried) my hopes of success were grounded, and, as the result showed, not without good cause.

I resolved to overcome the spasm of the muscles closing the jaw by throwing into action those whose function it is to open it, and which, in comparison

with the former, were feeble and ill-nourished. Also it was determined to arouse the muscular contractility of the bladder, weakened by long abeyance of its activity and by the distension of its walls from long retention of urine. As the patient had been saturated with nervine tonics of every description for many years, she was to take nothing but a little tincture of sumbul in camphor-water occasionally.

The first application of the Faradising instrument to the depressors of the jaw was perfectly successful. The opposing muscles, oddly though it may sound, appeared taken by surprise, and yielded in a very few minutes with a sort of snap. I gave the patient a pretty strong dose of the electric current, and she was informed that it would continue to increase in its force every day until the mouth ceased to shut! Then with a moistened sponge on the conductors, the abdominal muscles were first acted upon. They were in a state of hysteric rigidity, and no doubt thus helped on the seeming paralysis of the bladder. On the right side there was much less contractility of these muscles—and, indeed, generally of the muscles of the body—than on the left, and this was developed in a striking manner by the varying tolerance of the electric current. There was also a most marked anæsthesia of the skin all over the right side of the body. By soldering on a small brass button of the size of a small pea to the end of a piece of brass wire, and then enclosing this in an elastic catheter, I manufactured an excellent instrument for carrying the electric current to the walls of the bladder itself without communicating it to the urethra, which would have been intolerably painful. By the moistened sponge conductor applied over the pubis, and by connecting the wire of the catheter with the other pole of the instrument, we succeeded in arousing the torpid muscular irritability of the bladder. This was the treatment on which I based my hopes of an ultimate victory over the bladder, and it proved in the end quite successful. At the same time, having removed the irritability of the urethra, I determined to overcome temporarily its contractility, by gently dilating it as in the removal of stone from the female bladder, and thus break the habit of retention of urine. This was effected under chloroform by Weiss's dilator, and excellent results ensued.

In a short time an obvious improvement took place in the patient's general state, and in her increased muscular powers. The depressors of the jaw acquired strength and a fuller increase of volume, and there was a very marked increase in the muscular power of the bladder, so that at last on using the catheter it projected a forcible stream, instead of the urine coming away in a mere passive flow. Occasionally a parenthesis of hysteric spasm would occur in the muscles of the face or of the neck. One morning my patient came to me with a mimic torticollis, and another time with her face drawn to one side in the most grotesque fashion. In the manner of a very decided rebuke for such unreasonable conduct, she received on each of these occasions so strong a dose of the electric current as to put an end to the difficulty as if by magic! We were thus gradually dislodging this martyring disorder, by, at the same time, giving her the muscular power to vanquish it, and calling out the will to that end. Much patience and a most determined resolution to conquer—at first wholly on my part, but afterwards very decidedly on that of the sufferer—brought this strange case to its close. The last thing done for her was on Saturday, the 9th of June, 1860, and on Monday, 11th, she came, overflowing with gratitude, to announce herself set free from her disorder.

It is now nearly two years since this lady was discharged cured of her hysteria, and from that time to the present not even the shadow of any of her former annoyances has fallen over her history. Considering the long

duration of her disorder—more than thirty years—its extraordinary pertinacity and obstinate recurrence, we may fairly conclude two years' absolute immunity to represent a complete cure. And as the menstrual function is now closed, and a source of irritation thus removed, we may fairly hope that this lady, who was called by some of the eminent physicians who attended her the "Queen of Hysteria," has vacated her throne to some younger successor.

ART. 43.—*An Extraordinary Case of Chorea.*

By Dr. W. CANNIFF, Professor of Surgery, University of Victoria College, Toronto.

(*British American Journal*, November, 1862.)

CASE.—Jane M., aged 23, a native of Limerick, has been married nearly three years. Three months after marriage had a miscarriage, which, according to her own and friends' account, took place without any obvious cause, unless it were general weakness. Three months thereafter had a second miscarriage, still without any assignable cause. Fully nine months after this she had a third miscarriage, or premature labour. It was on this occasion that she first came under my notice, having been called in haste. When I arrived I found a still-born child still attached, and the placenta in utero. The child had apparently been dead for some time, and was between six and seven months. The placenta soon came away, and she had a quick and perfect recovery. The patient and her friends assured me that her health had been invariably good, and that no cause of the death of the child was known. About four months after this the patient found herself again pregnant. Two months later she had a severe fright from seeing a scuffle between her husband and father. This affected her mind very much, causing great depression of spirits, and at times great terror. This continued for about six weeks, when symptoms of chorea presented themselves, at first in the fingers and hand of the left side, and afterwards on the whole of the same side of the body. I recommended the utmost gentleness and cheerfulness to be observed towards her, and put her on the citrate of iron and quinine; under this treatment she gradually improved, until at last the twitchings had almost ceased, when, without any particular cause, the whole of the right side became affected, rendering her unable to walk or help herself. The treatment heretofore pursued seemed to have no effect, and I was induced to try the citrate of iron and strychnia. She very soon began to improve, and, although her progress was slow, she in about three months was perfectly free from the disease. She continued quite well until she had gone her full period of pregnancy. But with the labour-pains the chorea returned in every part of the muscular system. The several stages of labour were passed through in a comparatively short time; but as the labour advanced, and the pains became more severe, the jactitations increased in frequency and power. To attend a woman in confinement who has St. Vitus's dance is anything but easy, while the attitudes, the grimaces, and contortions were absolutely ludicrous. The patient was heartily ashamed of herself, and tried fruitlessly to keep in "position." In spite of herself, and mother, and nurse, and doctor, she would be first on one side of the bed, then on the other; now up against the head-board, now down to the foot. I had no particular fears until the head began to press against the perineum, when I thought, notwithstanding support and actual pressure, the head would be thrust through the undilated tissue. At each pain the urine would be forced out

in quick streams from the urethra, while the sphincter ani could be felt twitching most actively. The labour was fortunately completed without any evil results. The twitching at once began to cease, and within twelve hours had entirely disappeared: her recovery was good. The child, a fine boy, was healthy, and has continued so up to the present time, now nearly a year. The mother also remains in good health.

ART. 44.—*Acute Chorea treated with Whisky.*

By Dr. CLARK.

(*American Medical Times*, August 2, 1862.)

CASE.—A girl, æt. 16, was admitted recently into the Bellevue Hospital, New York, with symptoms of aggravated chorea of four weeks' duration. The convulsive movements were so violent and incessant, that she had not slept for four or five nights previous to her admission. Dr. Clark having observed the sedative effects of whisky administered in intoxicating doses in some cases of idiopathic tetanus, determined to try the same remedy in this case. He directed that half an ounce of whisky should be given to the patient every half hour, until intoxication was produced, (if necessary,) and sleep followed. After the third dose the girl slept half an hour; after the fourth dose she slept three hours; and from this time she slept well with doses repeated at longer intervals, and subsequently with the use of tonics she improved rapidly. There were no symptoms of heart disease, and no history of previous rheumatism.

ART. 45.—*The Degree of Frequency of Diphtherial Paralysis.*

By Dr. HENRI ROGER, Physician to the Hôpital des Enfants Malades, Paris.

(*Archiv Gén. de Méd.*, Jan., 1862; and *Edin. Med. Journal*, June, 1862.)

Without going beyond the facts already recorded, it might be said in a general way, that of all the paralyses secondary to acute affections, none is nearly so common as that which we see developed during the course of, or during the convalescence from, croup, and especially from pharyngeal diphtheria. M. Maingault, in a work on diphtherial paralysis, published in Paris in 1859, records ninety cases of the kind, which, indeed, are spread over several years, were partly collected by himself, but chiefly borrowed from various authors, and are, besides, selected cases. Since attention has been directed to this complication of diphtheria, the examples have become infinitely more numerous; but there is in this greatly increased proportion at once an appearance and a reality: the paralysis, no longer passing unperceived as formerly, appears to be more common, and no doubt really is so; for epidemics of diphtheria are certainly more frequent, more general, and perhaps more severe now than they were thirty years ago. This is the opinion of Professor Trousseau, who attributes the large number of cases of consecutive paralysis to this, "that for some years back, diphtheria has assumed that peculiar physiognomy which it did not formerly possess, and which characterizes the toxic form." (*Clinique Médicale*, tom. i., p. 380.)

"During the last few years the medical journals have published more than one hundred and fifty cases of diphtherial paralysis, and, as Dr. See remarks, 'in certain epidemics, the third or the half of the patients have been the subjects of an affection of sensibility or motility.' M. Lemaire, of Pont-Audemer, treated in the course of a few months eighteen cases of pharyngeal diphtheria, of which six terminated fatally, and every one of the twelve patients who recovered suffered from a consecutive paralysis.

"On comparing the preceding statements of different observers, we only arrive, however, at a somewhat vague approximation to the real proportion of cases of paralysis consecutive to pseudo-membranous affections; and if we have a certain idea of the *absolute* frequency of this complication, we have no data for deciding as to its *relative* frequency: this proportion has not been numerically determined, and I propose to endeavour to establish it, and to bring more precision into this question of etiology by means of clinical and statistical researches.

"In the first place, I shall quote from two documents having reference to hospitals with which I was not personally connected.

"During the first half of 1859, four cases of general diphtherial paralysis presented themselves at the Hôtel Dieu in the wards of Professor Trousseau; in two of these cases the diphtheria had been treated outside, and the patients had been brought to the hospital on account of the paralysis; in the other two the paralysis occurred in the hospital consecutive to a diphtheria of which all the phases had been observed. During the same period the number of diphtherial affections treated in the wards of M. Trousseau amounted to twenty-two, distributed as follows:—croup, 12; pharyngeal diphtheria, 6; cutaneous diphtheria, 4. Here, then, comparing the two cases of paralysis which occurred in the hospital with the twenty-two cases of diphtheria treated, the consecutive paralysis would be in the proportion of 2 to 22, or one-eleventh; but if it be borne in mind that in the series of observations the paralysis only occurred after pharyngeal diphtheria, which was only met with six times, the frequency of paralysis after pharyngeal diphtheria becomes greatly augmented, amounting, in fact, to one-third.

"In the course of the same year, 1859, M. Garnier collected at the Hôpital St. Eugénie, in the wards of Dr. Barthez, fifteen cases of diphtherial paralysis; of these fifteen cases, six were admitted into the hospital on account of paralysis consecutive to diphtheria, for which they had been treated at their own homes; in the other nine cases the whole course of the affection was observed, but M. Garnier does not specify (except in two cases of cutaneous diphtheria) to what form of pseudo-membranous affections the paralysis had succeeded. As M. Barthez received into his wards a hundred and forty-one children suffering from diphtherial affections, this gives a proportion of one case of paralysis to about fifteen of diphtheria in general.

"It occurred to me that, in order to be able to form an accurate idea of the relative frequency of the affection, it was necessary to observe what happened in cases of croup or pharyngeal diphtheria,

under circumstances which permitted the observation of the disease during all its periods, and where there was an opportunity for witnessing the development of the paralysis. Accordingly, I have collected all the cases of diphtheria, more or less generalized (whether in the form of pharyngeal diphtheria, or of laryngeal, tracheal, or bronchial), which occurred in the wards of the Sick Children's Hospital during the year 1860. The number of these cases is 210.

"The cases of diphtherial paralysis observed during the same year were 36 in number; but as 5 of these cases only entered the hospital after the cure of the primary affection, and solely on account of the paralysis, I exclude them from my calculation as to the frequency of the affection, although they will be taken into account when I come to consider the clinical history of the affection. There remain, then, 31 cases of paralysis (either general, or, as most frequently happened, limited to the pharynx), out of a total of 210 cases of diphtheria, which gives us a proportion of about one-sixth, or nearly 15 per cent. But this proportion, already considerable, must be materially augmented; because, on the one hand, some of the children who quitted the hospital as soon as convalescence was fairly established may have been lost sight of; and, on the other, in a very large number who died during the early stages of the disease there was no time for the establishment of consecutive paralysis; thus, out of 104 children in whom tracheotomy was performed on account of croup, 13 died on the day of the operation, 31 on the second, 22 on the third, consequently more than the half before the fourth day; that is to say, among the children so rapidly carried off, there was no time for the development of a secondary affection, such as a diphtherial paralysis; and it was only in a small number of the cases, where death occurred from the primary disease, that life was sufficiently prolonged to give the local phenomena of paralysis an opportunity for manifesting themselves. Although the object of my researches is to found upon statistics as the surest basis of my opinion as to the frequency of paralysis secondary to diphtheria, I conceive that, for the reasons I have given, I am entitled to assume a higher proportion than the mere numbers would convey, and that, from a calculation of probabilities, I am justified in raising it to a fourth or even a third, instead of a sixth.

"I have proved in a direct manner, by facts carefully observed, that paralysis is a very frequent sequela of diphtheria; it is not sufficient to allow this to remain as a barren fact, but it must henceforth be looked upon as an important characteristic of pseudo-membranous affections; and since this paralysis has a special cause, as well as peculiar symptoms, course, and termination, a special place should be assigned to it among paralytic affections, just as, among dropsies, scarlatinal dropsy is specially recognised. I shall now show, by indirect proofs, that this really deserves to be considered a special form of paralysis.

"The result of my statistical inquiries permits me to state in the clearest manner, that secondary paralyses are as rare after other acute diseases as they are common after diphtheria. I do not absolutely deny that paralysis may supervene during convalescence from

febrile affections, and may be connected with them in some unknown manner; in the case of typhoid fever and simple pharyngeal angina, the fact appears positive, although one circumstance must be kept in view, namely, that in angina, accompanied with slight symptoms, it is easy to overlook the existence of a scanty false membrane which has soon dropped off, or which, limited to the posterior surface of the uvula and the pillars of the fauces, has escaped attention—in other words, that some cases, supposed to be cases of simple angina, are really cases of diphtheria. In the same way, in cases of typhoid fever, if the throat be invariably examined, it will be found that there almost always exists an erythematous angina of the vault of the palate and the pharynx, and which is complicated more often than is generally supposed with the secretion of a pultaceous, and even fibrinous matter—in fact, a pseudo-membrane of about the same consistence as in muguet: I am, therefore, disposed to explain, by the antecedent existence of an angina of this nature, the small number of cases of paralysis of the vault of the palate, and more general paralysis, which have latterly been observed to follow dothenenterite.

“I admit the possibility of paralysis consecutive to acute diseases: in twelve years of private practice, I have seen four cases of secondary paralysis, one in a little girl after ataxic pneumonia, another after double pleurisy, and the other two in young women after hepatitis depending upon biliary calculi; but the *frequency* of this phenomenon is so inconsiderable, the occurrence is so exceptional, that etiologically I attach no value to it. I again refer to statistics in reference to this point. During the same year, 1860, I took a note of all the children, boys and girls, who were admitted into my wards, with an exact indication of the diagnosis and result; in no one child, no matter whether the disease was acute or chronic, slight or severe, did I notice any paralytic phenomena, except in cases of diphtheria and well-characterized cerebro-spinal diseases. Thus limiting myself to diseases, as sequelæ of which paralyzes are said to manifest themselves, I subjoin the figures relative to simple angina, pharyngeal or laryngeal, typhoid fever, the eruptive fevers, and pneumonia. Of simple angina, there were 61 cases; 40 of pharyngeal angina; 21 of laryngeal. Of typhoid fever, 12 cases. Of measles, 33 cases; of scarlatina, 12; of variola or varioloid, 4. Of pneumonia, 24 cases. In none of these was there any secondary paralysis.

“It would be easy for me to add to these figures, by giving the precisely similar results of my colleagues, but this appears to be unnecessary. Returning, therefore, to my starting point, and considering, on the one hand, the excessive rarity of secondary paralysis in the phlegmasiæ, and in fevers other than dothenenterite, and, on the other, its excessive frequency after diphtheria, I believe that it is consistent with a sound pathology to establish a special class of diphtherial paralysis, and not to allow them to be lost in a vague group of secondary paralysis.”

ART. 46.—*On Diphtherial Nerve Affections.*

By Dr. E. HEADLAM GREENHOW.

(Proceedings of the Royal Medico-Chirurgical Society, March 24, 1863.)

The epidemic sore-throat which, under the name of diphtheria, has latterly engaged so much attention, is well known to be followed by nervous phenomena of a peculiar kind. These consist chiefly of impaired, excessive, or perverted sensibility, together with more or less complete paralysis of the muscles of the fauces, pharynx, tongue and lips, extremities, trunk, and neck; the frequency of the occurrence of these symptoms in the several sets of muscles being nearly in accordance with the order in which they have just been mentioned. The author has had the opportunity of watching the course of several cases of these diphtherial nerve affections in patients under treatment at the Middlesex Hospital, and the present paper is in a great degree based on these observations. He does not mean to infer that every attack of diphtheria is followed by some of these secondary nerve affections, for he has seen patients recover perfectly without experiencing any of them; nor to assert that their intensity is always proportioned to the severity of the primary disease, for he has sometimes seen them follow comparatively mild attacks of diphtheria. Nevertheless, as a general rule, he has certainly observed these nerve affections to be more frequent after the worst cases of diphtheria, and to bear some proportion even to the local severity of the attack; he has noticed, for instance, that the paralysis and anæsthesia are sometimes more complete on that side of the fauces which had been most severely affected by the primary disease. The author has found that a brief period of convalescence—generally not exceeding a few days, but in rare cases extending to weeks—almost always intervened between the disappearance of the sore-throat and the accession of the nerve symptoms; and cases have fallen under his notice in private practice in which patients who had recovered sufficiently from diphtheria to be sent from home for change of air, had subsequently fallen into a helpless condition from diphtherial paralysis. The fact of this interval seems to him important, inasmuch as it goes far to show that the paralysis could not be entirely attributable either to the albuminuria which so often accompanies the acute stage of diphtheria, or to the anæmia which closely follows it, as patients have often got rid of the former symptom, and even begun in some cases to regain flesh and strength before the accession of the paralytic symptoms. The author has observed that these nerve affections do not at once attain their maximum of intensity, but are progressive even in the same sets of muscles; and also that if several of the sets of muscles which he has enumerated should be attacked in the same individual, they do not become affected all at once, but in succession—the faucial or pharyngeal muscles being the first to suffer, and so on in the order in which he had placed them at the beginning of the paper—though it by no means follows that all of them should be affected in any one

case. He has found the muscles of the fauces by far the most frequent, as well as the earliest, seat of nerve affections after diphtheria, and has seen them attacked in many cases in which the rest of the muscular system either entirely escaped or was very slightly affected. When the fauces are paralysed, the soft palate lost its natural action, the speech often became imperfect, and liquids regurgitated through the nostrils. These symptoms should be discriminated from the hoarseness of voice and return of fluids through the nostrils which often occur during the acute stage of diphtheria, and arise, as in ordinary quinsy, from the swollen and painful state of the fauces impeding the natural action of the parts. Anæsthesia has co-existed with the paralytic affection of the fauces in all the cases that have come under the author's notice, so that these naturally very sensitive organs became altogether callous and insensible to touch. Next to the affection of the fauces, impairment of vision, probably due to paralysis of the ciliary muscle, appears to be the most frequent of the nervous disorders consequent on diphtheria. The author has observed that the pupil of the eye became dilated, and acted sluggishly under the influence of light a day or two before the sight became sensibly impaired, and often remained so for a time after the sight had been regained; also that patients unable to read with unassisted sight could do so with the help of convex spectacles, and hence he attributes the impairment of sight to a temporary loss of adjusting power. The nerve symptoms which he has noted in the tongue and lips were formication, or a sense of scalding, numbness, and impaired taste and power of movement. They began, for the most part, in the lips and the tip of the tongue, and gradually extended upwards towards the dorsum and root of the latter organ. The limbs suffered more or less in all the five cases which formed the basis of the paper, from paralysis and anæsthesia, besides tenderness and abnormal sensations, such as coldness, formication, and a feeling of constriction in the fleshy parts, as if they were tightly bandaged. These affections began either first in the upper, or at the same time in both the upper and lower extremities, and were at their commencement peripheral, extending gradually upwards from the tips of the fingers and toes towards the trunk, and in some cases affecting the lower part of the back and of the abdomen. He has found that pressure over the sciatic and median nerves was sometimes attended by acute pain, and that pressure of the instep between the finger and thumb sometimes caused convulsive starting of the leg and foot as well as pain. He has observed that the paralysis in some cases assumed a more or less hemiplegic character, but has seen no instance in which, one side being paralysed, the other remained entirely unaffected. The author has seen nerve affections after diphtheria of a graver character than any of those exemplified in the present group of cases, and several even fatal cases had fallen under his notice in private practice. In three of these latter death was caused by failure of the action of the heart, and in one by exhaustion from vomiting. He believes, however, that such cases were fortunately exceptional, and that the great majority of sufferers from diphtherial nerve affections, under good management, sooner

or later recover their usual health and strength. The author has satisfied himself that these cases were best managed on sound general principles. Generous diet and a liberal allowance of stimulants, together with rest in bed, he believes to be always necessary. Tonics, especially steel and quinine, or the mineral acids, he has found useful from the first appearance of the nerve affections; and after the complete development of the paralytic symptom, nuxvomica and strychnia have proved in his hands most valuable remedies. The remarks in the paper are founded on five cases.

ART. 47.—*Clinical Remarks on Cases of Infantile Paralysis.*

By Dr. WILKS, Physician to the Royal Infirmary for Children.

(*Lancet*, April 18, 1863.)

The subject of infantile paralysis having of late been before the profession, the following short report of cases of this disease may be of some interest. Amongst out-patients it is almost impossible to take a very full account of their histories, and much less possible is it to watch the cases to their terminations. The following short notes, however, all refer to a question which has been lately much discussed—whether there be any accompanying symptoms referable to a change in the cerebral or spinal centres. It will be seen that the cases corroborate the general professional experience, that although very often there is evidence of some general nervous derangement, yet that is by no means always the case. It is for this reason—the absence of all lesion—that the name *essential paralysis* has been given to it by the French. In the following cases, as in others, there is sufficient to suggest some central cause, yet this must not be assumed in those where no history was obtainable indicative of such disturbance. We think it must not be assumed, in the case of a child previously in good health in whom an arm or a leg is found paralysed, that the child *must* have had a convulsion, followed by a general paralysis, and in whom both the attack and recovery were unknown to the mother, and only, indeed, suggested to the medical man by the remnant of paralysis which he finds. The subject is one of the most interesting in nervine pathology; but, considering that perfect recovery sometimes takes place, it is too much to assume that an organic lesion must always have occurred. A fatal result seldom takes place; and hitherto, we believe, no post-mortem examination has been made to decide the point. It is scarcely necessary to allude to the absurd popular opinion of attributing every child's malady to the teeth, whether it be nervous, cutaneous, or gastric. That great changes occur in the body about the period of dentition, every medical man knows; but that the direct process of tooth-cutting has anything to do with the disorder, there is no proof, although we are ready to admit that much may be due to reflex action.

It will be seen that in some of the following cases the child has gone to bed well, and been taken up in the morning paralyzed in

the arm or leg. We have surmised whether in such cases the paralysis may not have been purely local, and due simply to pressure on the nerve of the limb, or possibly sometimes to cold applied to the surface, in the same way as facial paralysis may arise from a *coup de vent*. We have thought this, because in many cases we have been unable to trace any cause whatever for the attack; and we know, indeed, one instance where a child in good health, having sat for a considerable period at a window with his leg doubled under him, had the limb quite benumbed and rendered helpless in consequence, so that he was obliged to be carried to bed by the nurse. On the following morning the leg was found still paralyzed, preventing the child from standing. He gradually recovered by sea-bathing in the course of two or three weeks. If the occurrence had not been observed by the nurse, the cause would have remained altogether unknown to the parents of the child. We see no reason, therefore, why the paralysis may not in some cases have no more deep-seated cause than accidental pressure. In others a blow may, we think, be the starting point of the disease.

It has been said that this affection is connected with club-foot, and some have described them together; owing, however, to the divided practice of the physician and surgeon. perhaps neither the one nor the other is the fittest person to decide the question. But, for our own part, we have never been of opinion that the two diseases are pathologically associated. Talipes we have regarded as a result of spasm of the muscles, connected often with general convulsions, and arising from nervous irritation during either intra or extra uterine life; whereas infantile paralysis, if not cured, does not, so far as our experience goes, terminate in spasm, but in atrophy of the limb: the muscles waste, and the whole extremity becomes smaller or ceases to grow with the rest of the body; the consequence being that the limb hangs helplessly at the side, and in the case of the leg often obliging the patient to use a crutch, as the enfeebled limb does not reach the ground. Without examining the limb, the impression might be that such a patient had suffered from hip-joint disease in infancy. If a child with club-foot be brought to a surgeon, and a history of convulsions followed by a contraction of the leg be related, we should not regard this as a case of infantile paralysis; we believe the pathology of the two is different, and a different treatment is required.

A certain proportion of cases recover, although we cannot state what that number is, for reasons already given. We have never found any particular medicine which can be styled a remedy; but tonics, and all means which will stimulate the limb, are good—as rubbing it, exercising it as much as possible, and galvanism. The latter we have seen very serviceable; for if it acts in no more direct manner on the inert functions, it stimulates the muscles and prevents their waste. The only drawback to this remedy is the dread experienced by the child, which is sometimes so great that it cannot be persevered with. Of course, if there appear to be any special cause for the paralysis in the nervous centres, in the bowels or teeth, appropriate remedies will be given.

CASE I.—Jane F——, aged twenty months. Well up to six months ago, when she had convulsions, and very soon afterwards it was observed that the right arm was weak. There was now almost total paralysis of motion in this arm; child otherwise quite well. Occasional alterative powders of the soda-with-mercury powder, embrocations, and galvanism, were prescribed. The child left at the end of a month, when the arm had made a decided improvement.

CASE II.—Nelly S——, aged seven months. A month ago the mother noticed that the child had quite lost the use of the right leg. Now it was found perfectly paralyzed, and dangled about just as it was moved. The child was very well, and the mother said it had never had any illness whatever, nor any trouble with the teeth. Ordered the same remedies as in the case above. Improvement slight. This child was seen a year afterwards, and the right leg was still weaker than the other.

CASE III.—James H——, aged a year and a half. Two weeks ago, when taken from his bed, the mother noticed that he could not stand on his right leg. Had no other symptoms; health good, and always had been so. Leg to be rubbed, and electricity used. Improving when left.

CASE IV.—Henry B——, aged three years and a half. This child had been at the infirmary various times during the previous two years, and exemplified very well the consequences resulting from this affection if not cured. The left leg during this period had been partially paralyzed; the limb was much smaller than the other, and so weak that the child could with difficulty bear his weight upon it. He could, however, just walk. As galvanism had never been used, it was ordered; but the patient was so alarmed by it that it was obliged to be discontinued. He was under notice for some weeks, but had not much improved.

CASE V.—Sarah B——, aged a year and a half. The mother noticed that the child was unable to move her arm for the last few days. The child was in perfect health, and no local cause (as injury) could be discovered. She remained until the card had expired, when the arm had slightly improved in strength, but not to any great degree. The child's health in other respects was excellent.

CASE VI.—Thomas M——, aged two years and a half. Paralyzed in the left leg; could move it slightly when in the mother's arms, but could not stand upon it. The history was remarkable, it being stated that the child fell and struck his leg, and on the following day it was perfectly helpless. On careful examination, no signs of any injury could be discovered, but the case presented the features simply of infantile paralysis. At the end of a month the child had improved considerably.

CASE VII.—Henry S——, aged eleven months. Three months before, he was taken unwell and had slight convulsions. It was afterwards noticed that the right leg was weak, and this has been quite paralyzed since. Galvanism, &c. At the end of six weeks he left in much the same state.

CASE VIII.—George P——, aged fifteen months. Two months ago the child was weaned, and soon afterwards became ill, but had no convulsions or symptoms of head affection. It was soon discovered that the right leg was paralyzed. Before this he could walk alone. Galvanized. Left at the end of the month no better.

CASE IX.—George L——, aged three years. Eight months ago was ill, and under medical care for his head, but had no convulsions. He was a long time ill, and the mother was unaware whether the leg was affected then or not, but only on recovery four months ago did she discover that he was unable to walk on account of weakness of the right leg. Has been unable to walk since. Child only came twice.

CASE X.—William C—, aged twenty months. Three weeks ago the child was ailing, which the mother supposed was due to the teeth. Had no fits. Soon afterwards it was observed that the left arm was utterly powerless. The child is delicate, with a large head. Ordered embrocation, use of arm as much as possible, and afterwards galvanism. At the end of a month the arm was much stronger, the child could grasp with it, and the case was evidently doing well.

CASE XI.—Charles F—, aged two years. Been paralyzed several weeks in the right leg. No cause could be discovered for it. Galvanism, &c. In the course of six weeks the leg had so far improved that the boy could stand upon it, but he could not yet walk.

CASE XII.—Anne F—, aged one year and a half. Four months ago she had an illness, the nature of which the mother is not aware of; but when she had recovered, it was found that the child had lost the use of her right leg. The limb is now quite powerless, dangles about as moved, and muscles wasted. Galvanism, &c., ordered. At the end of two months, when left, some improvement had taken place.

Since this form of paralysis is usually met with in young children, it has been considered to have a special pathology. It may be only true, however, that patients at this time of life are more liable to a morbid condition, which may occasionally occur at any age.

CASE XIII.—James R—, aged fifteen. Three months ago he was unwell. Slightly feverish, and kept his bed a week. On getting up he found his right arm so feeble that he was scarcely able to use it. He now cannot raise his arm from his shoulder; but when resting it on a table can use the muscles of forearm and fingers. Can discover no local cause for the paralysis, and the boy is in excellent health. Galvanism ordered. When he left at the end of six weeks he was considerably better.

The above cases do not, unfortunately, throw much light upon the pathology of the disease; but some of them do show that the statement of the necessary existence of a cerebral lesion is an assumption. In none of them, whilst under treatment, was there any contraction of the tendons commencing to take place.

ART. 48.—*Case of Progressive Muscular Atrophy of the Hands, with Enlargement of the Ventricle of the Cord in the Cervical Region (Hydromyelus), and Atrophy of the neighbouring Grey Matter.*

By Dr. GULL.

(*Guy's Hospital Reports*, 3rd Series, Vol. VIII. 1862.)

This case is given as a contribution to the pathology of progressive muscular atrophy, and as suggesting a doubt respecting the validity of the present theories of the function of the grey matter of the cord. Although there were no other remains of the grey matter in certain parts of the cervical region but the inferior cornua, the patient was still able to walk perfectly well, and to move the arms freely in all directions, and the sphincters acted properly; *nor was there any affection of sensation in any part, except a feeling of numbness in the right hand.* The case, which seemed to be a chronic

hydromyelus, comparable to a chronic *hydrocephalus*, is illustrated by drawings.

CASE.—G. B., æt. 44, a journeyman tailor, of sober habits, was admitted into the clinical ward, February 5th, 1862. States that he has always been healthy and strong. Never had any injury to his back. Thirteen months ago, when working in London, the fourth and little finger of the right hand became weak and flexed, without any assignable cause. The hand was cold, and there was a feeling of numbness in the fingers, but no pain. This gave rise to a good deal of inconvenience, but he was able to continue working at his trade. Two months ago, the middle finger of the same hand became suddenly affected, and three weeks ago the three inner fingers of the left hand became weak and flexed in the same way, but without any numbness. The hands gradually wasted. The arms are not affected. Seven weeks ago he had pains through his chest, and a feeling of tightness across the upper part. He is pale, complains only of wasting and weakness of the hands; has no pain in them, but the right is cold, with a feeling of numbness. The left hand is not so cold, and the sensation in it is perfect. He can move both thumbs and index fingers freely; he can also extend the first phalanges of the other fingers of both hands, but not in the least degree the second and third phalanges, which are gently flexed towards the palm. The interosseous spaces on the backs of the hands are sunken from the wasting of the muscles. The palms of the hands are hollow, and the flexor tendons very prominent. The thenar eminences are wasted, and the hypothenar almost entirely gone, particularly on the right side. The motion of the wrist joints is unaffected. He can move the arms freely in all directions. Can walk perfectly well.

At the upper part of the dorsal region there is a slight flattening of the natural curve of the spine, from the long muscles of the back being at this part wasted. Pressure on the fourth dorsal spinous process causes a sharp, pricking pain, as of a knife running into the part, but when the part is not touched he has no pain. No pain on pressing the other spinous processes; no affection of sensation in any part, except the feeling of numbness in the right hand; sphincters good; urine normal; appetite and digestion good. He was put upon a full diet, and the wasted muscles were daily galvanized by an intermittent current. A fortnight after admission he had gained power in the hands. He said he felt them stronger and more pliable after each application of the galvanism. It was noted that, with a moderate current, the contractility of the muscles of both hands was good, but more particularly in the short muscles of the thumb, which were least wasted. Sensibility not so acute in the right hand as in the left, but no marked anæsthesia of either. Both hands were rather cold. A few days after this report the patient sickened with typhus fever, and died on the 8th of March.

A post-mortem examination was not permitted at the hospital, and it was only after much difficulty that the cord could be obtained. The bones and ligaments of the spine were healthy; the membranes of the cord healthy; the exterior of the cord presented nothing abnormal, except that the cervical enlargement appeared broader and somewhat flattened. On making transverse sections, the white columns had their normal consistence and texture, but the centre of the cord had a large cavity, beginning at the fifth cervical, enlarging downwards to the seventh, and ending at the fourth dorsal. The only remains of the grey matter were at the anterior part of the cavity behind the anterior columns. Here the caudate vesicles had their normal size and structure; the pigment, nucleus, and nucleolus being well marked, and the tubular structure unaltered. The cavity in the cord was bounded

by a layer of condensed grey substance, which could be separated as a distinct membrane. On its interior surface, forming the lining of the cavity, were a number of delicate, elongated, nuclear bodies, apparently epithelium. One or two granule cells were found scattered amongst the white columns, but no further traces of any active tissue change. The roots of the nerves appeared normal, and contained healthy tubules. The character of the fluid filling the cavity could not be ascertained, as it escaped in the removal of the cord from the spinal canal.

ART. 49.—*Progressive Muscular Paralysis of the Tongue, the Curtain of the Palate, and the Lips.*

By M. TOMMASI.

(*La Sperimentale*, t. x., 1862; and *Gazette Hebdomadaire*, Nov. 14, 1862.)

Dr. Tommasi relates a well-marked case of this disorder which occurred in the wards of Prof. Pellizzari, of Florence, and which formed the subject of an excellent monograph read at a meeting of the *Medico-fisica* Society of that place. The importance of the case induces us to give it at some length.

CASE.—On the 30th April, 1862, a countrywoman, named Annonziata Maffai, aged 48, entered the hospital of Santa Maria Nuova, representing herself as suffering from a tumour of the pharynx. Having previously enjoyed good health, she suddenly perceived, in the beginning of September, 1861, that certain movements of the tongue had become difficult, especially those which in the pronunciation of certain words involved the contact of the tongue with the palate or teeth. At the end of the same month, she had an attack of rheumatism in the muscles of the right side of the neck and over the scapula, which was cured by the application of blisters. During this attack, the difficulty and imperfection of the movements of the tongue increased. Soon after, deglutition became difficult, and later still the movements of the lips began to be slow and uncertain.

The 5th May, 1862, the following was the state of the patient :—

The lips were of ordinary size and colour. When the mouth was closed, there was a little irregularity towards the free edge of the upper lip. The opening of the mouth was accompanied by an irregular and oscillating movement of the lips.

In the movements occasioned by talking, sucking, whistling, kissing, the contractions were slow, difficult, and always incomplete. If a piece of solid food penetrated between the lips and the teeth, it could not be removed without the assistance of the finger. The mucous membrane of the mouth was of normal colour and appearance. The tongue was of ordinary size; it had lost its convex form, and was soft, depressed on the median line, and raised towards the edges. The movements of the tongue were tolerably quick and easy. It could not, however, be put out freely, and was agitated by a slight tremulousness.

The curtain of the palate was relaxed and descended lower than usual, and the concavity of its anterior surface was diminished. The uvula came in contact with the base of the tongue; the contact of an irritant body occasioned only a slow and uncertain movement. During deglutition, morsels of food frequently intruded into the posterior cavity of the nostrils. The pharynx, on the contrary, had lost none of its contractility.

This irregularity of movement occasioned difficulty in the pronunciation

of certain words. All the vowels were pronounced tolerably well, with the exception of the *u*. Among the consonants, those which required the complete closing of the lips, such as *m*, *b*, *p*, or their opposition in a valvular form, such as *f*, *v*, were pronounced with difficulty. The letters which require the contact of the tongue with the palate were still more difficult to pronounce, as *g*, *th*, *x*, *q*; all were accompanied by an appreciable inspiration and a nasal sound. The sounds which require the application of the point of the tongue to the anterior part of the palate, as *c* (*ché*), *l*, *n*, *r*, or with the dental arch, as *s*, *l*, *z*, were scarcely intelligible. The sensibility of all these parts was normal, the sense of taste intact. The tactile sensibility only seemed slightly diminished on the curtain of the palate, and titillation with difficulty provoked nausea.

In exciting contraction by means of M. Duchenne's electro-magnetic apparatus, the orbicular muscles of the lips, the proper muscles of the tongue, the staphylins and glosso-staphylins, contracted as in their normal state, and the parts to which they distributed themselves regained their volume and their physiological form. The voice did not vary in strength; the laryngeal muscles contracted physiologically, but the vocal timbre had become nasal.

The physiognomy had not changed, and none of the facial muscles were paralyzed, except the two buccinators, which, in the action of blowing, separated themselves involuntarily from the dental arch. The motility and the sensibility of the rest of the body showed no alteration; the same remark may be made of the respiratory and digestive functions.

On the 6th May, the paralyzed muscles were faradized by the apparatus of M. Duchenne, conical conductors being placed especially in the course of the hypoglossal and facial nerves. At first, frequent intervals were allowed, but these were abridged by degrees, as the patient's power of endurance increased. The time of operation each day lasted about ten minutes.

Towards the 8th day, a slight improvement was perceived in the state of the orbicularis of the lips. From the 8th to the 15th, the amendment continued, the muscles of the tongue had more tonicity, the deglutition of saliva and of food was easier, the sound of the voice more distinct, though still quite nasal. About the 13th day, the pusillanimity of the patient made it desirable to diminish the length of the *séances*, and the strong current was exchanged for a feebler current. However, the improvement continued long enough to give hopes of a cure, and at the end of June the patient quitted the hospital.

Her state was then as follows:—The lips had regained their original form, their contraction being strong and regular, and the labial consonants being distinctly pronounced. The tongue was easily protruded to a certain distance; its movements had lost their uncertainty; and its tip could be elevated slightly above the level of the teeth. The palatal letters were pronounced without aspiration; the curtain of the palate was more irritable, its concavity had increased, and its inferior border become considerably raised. Deglutition was prompt and easy—nothing returned through the nose; but the sound of the voice was still nasal.

The progressive muscular paralysis of the tongue, of the *velum palati*, and of the lips, is a very serious malady, to which attention has been recently called, for the first time, by M. Duchenne of Boulogne, and of which there are as yet very few cases on record.

ART. 50.—*Some Remarks on the Causation of Organic Paralysis.*

By Dr. C. HANDFIELD JONES, Physician to St. Mary's Hospital.

(*British Medical Journal*, November 15, 1862.)

In a paper on the physiology of the spinal cord and brain, in the *Journal de la Physiologie*, Dr. Brown-Séquard adduces some important facts, which go to show that irritation of sensitive or excitomotor nerves produces, for a variable time, a diminution of the vital properties and functions of the part of the spinal cord to which they proceed. Two of the most striking are the following:—4. The application of a ligature on the hilus of one of the kidneys, or suprarenal capsules, or, in other terms, the irritation of the nerves of these organs, determines very often the same effects as section of a lateral half of the spinal cord. 6. If, after having rapidly exposed the cord in the dorsal region, without having produced paralysis or anæsthesia of the posterior limbs, we excite the posterior roots of a pair of nerves on both sides, we observe immediately paralysis and anæsthesia in the posterior limbs; and if we irritate several pairs, these results increase in proportion to the number of pairs irritated and the degree of irritation.

In a preceding paragraph, Dr. Brown-Séquard states his intention to demonstrate "that it is by a reflex action on the blood-vessels of the nervous centres that irritation of centripetal nerves (sensitive or excitomotor) determines the alterations of nutrition, as the result of which are produced very often paralysis, anæsthesia, and different forms of convulsive affections (hysteria, epilepsy, catalepsy, chorea, tetanus, cramps, contractions, tremblings, &c.)."

Now, as the paralysis from irritation of centripetal nerves is said to occur immediately, it seems difficult to believe that any notable amount of alteration of nutrition can occur so rapidly, at least as the result of mere anæmia. Animals after decapitation, when the circulation must be wellnigh abolished, manifest reflex movements perfectly; anæmia is therefore insufficient to account for the loss of function of the cord. Dr. Brown-Séquard's own reply, in the same paper, to some objections against certain other experiments, seems conclusive on this point.

"It is true that the cord loses one of the sources of its supply of blood when the roots are divided; but, on the one hand, the quantity of blood which the cord ceases to receive after the section of seven or eight pairs of nerves, is not very considerable (P); and, on the other hand, the entire suspension of the circulation after the removal of the heart allows the properties and functions of the cord, not separated from its roots, to remain during one or two minutes, whilst the section of the roots annihilates or diminishes immediately these properties, and as quickly abolishes, or renders less active, the exercise of its functions."

It seems, therefore, incontestable that we must look to some other cause than local anæmia for the paralytic phenomena in these and

like cases. If we look to the two chief kinds of non-organic paralysis,—viz., that termed reflex, and that which our author has called simple, or neurolytic,—we see that in the first the palsy, to all appearance, depends on some morbid impression conveyed to the centre, ceasing on its removal; while in the other the nervous centre appears to be directly enfeebled, as by the action of some poison, or obscure influence on its tissue; and the “juvantia” are not, as in the other case, the removal of an irritation, but stimulants. Amaurosis from dental irritation, paraplegia from a stricture, are examples of the first; influenzal and malarious paralysis of the second form. In the latter affections the signs of general debility are often so *prononcé*, as well as the effects of tonics and stimulants, that we can hardly err in regarding the condition of the nervous centre implicated as one of debility. We are then entitled, it seems to me, to assume the occurrence of a form of paralysis depending on temporary functional disorder or exhaustion of a nervous centre. Some minute alteration in the cells of the grey substance, or in the connected axis-cylinders, would easily arrest the free passage of nervous force. Now, if this may occur from the direct influence of some poisonous matter in the blood, it seems at least probable that it may also occur as the result of some morbid impression on the periphery of a centripetal nerve. That the paralysis is in some way dependent on the morbid impression, we know; that anæmia from contraction of blood-vessels will not account for it, we have admitted; there remains, therefore, as it would seem, nothing but to assume some interference with, or derangement of, the minute molecular changes which occur normally during the active state of nervous tissue. In the foregoing remarks, Dr. Handfield Jones has assumed that the morbid condition termed reflex paraplegia is closely analogous with that produced in the experiments above cited.

ART. 51.—*A Case of Remarkably Profound Sleep.*

By Dr. J. W. COUSINS, Surgeon to the Royal Portsmouth Hospital.

(*Medical Times and Gazette*, April 18, 1863.)

CASE.—J. C., a farmer, aged 43, has been subject at intervals during the last twenty years to attacks of deep and prolonged sleep. He has never suffered from any disease of the brain, or any other illness. The disorder commenced without any assignable cause in the year 1842, and continued nearly a whole year. It returned again in 1848, and having persisted without interruption for eighteen months, it left him entirely for the space of twelve years. The present attack commenced on May 19, 1860. Since that time he has not slept naturally.

He retires to bed at night soon after 10 P.M., and almost immediately falls into a profound sleep, from which all the means at present adopted have failed to arouse him. He generally sleeps on his side, and appears like a person in refreshing slumber. His face and ears are pale; skin generally warm; but the feet are often cold and livid. Pulse slow and feeble; pupils generally somewhat dilated; respiration very gentle and shallow.

He seldom moves, but occasionally he turns over from one side to the other. He never snores or moans. He awakes suddenly, without giving any warning, and he always seems refreshed, just as if he had slept naturally. Occasionally he complains of a slight pricking sensation in the forehead.

The longest period he has ever passed in profound sleep is five days and five nights. Lately he has frequently slept three days, and occasionally four without waking, but the average time is nearly two days. He is awake about four or five hours out of forty-eight. During these remarkable sleeps he never dreams, and the contents of the bowel and bladder are always retained. Before he falls asleep, he says that he "sometimes feels stupid;" but this is the only head symptom he ever complains of. His memory is good. When he awakes he remembers everything that happened the day before he began to sleep, and always asks, "How long have I slept?"

Lately he has looked pale, and has lost flesh. His appetite is good, and the bowels are active. His manner is quiet and his disposition amiable. He is a good man of business, and is fond of reading. In intellectual power he is by no means deficient, but his early opportunities have been limited.

During the attack in the year 1848, he frequently suffered from spasmodic trismus, which generally commenced soon after he awoke, and persisted for many hours. His jaws were always firmly locked, and at the same time he complained of pain in the back and neck. This affection, however, has never reappeared.

ART. 52.—*A Remarkable Affection of the Nervous System.*

By MR. J. GRIEVE, of Port Glasgow.

(*Medical Circular*, March 4, 1863.)

CASE.—J. D., æt. 11, one of a family of six children, five of whom are still alive, and whose parents are apparently in ordinary good health, was, though previously in the enjoyment of good health, seized in the month of May, 1861, with slight shivering fits, but without any, or at least very slight, subsequent feverish excitement. During the succeeding twelve months she did not complain much, but nevertheless was rather unwell, the chief complaint being the want of appetite. She never could be prevailed upon to taste food of almost any kind; a little wine was occasionally forced upon her, but even from that she ultimately entirely refrained. At this time tonics of all kinds were tried, with change of air, sea-bathing, and every adjunct that possibly could be thought of. I may mention here that her residence is in the country, upon the banks of one of our magnificent Scottish forths, distant about five miles from any town. All the remedial means employed proved of no avail. About the end of May last, the first fit so characteristic of the disease occurred, and which has recurred with the most undeviating regularity ever since. I shall now endeavour to describe the paroxysms which affect her three times every day. The first comes on about nine o'clock in the forenoon. Up to within a minute or two of the fit setting in, the patient seems to be in ordinary health, and rather in good spirits, when suddenly she complains of being wearied, and invariably, whatever may be her previous position, she turns on her left side, and as if composing herself to sleep, and immediately becomes completely unconscious. She remains in this condition, from which no noise, pinching, or other means have as yet roused her, for two hours, and during the whole

of that time she never moves a muscle of her body in the slightest degree, but lies perfectly motionless. During this stage of the fit her eyes remain closed, with a slight tremulous motion of the eyelids. There is seemingly a continuous stream of flatus, as if by eructation puffing through the closed lips, which causes a quantity of frothy saliva to collect around the mouth; the abdomen is very much distended and tympanitic; the pulse ranges about from 110 to 120; the extremities, both hands and feet, feel rather cold to the touch; and there is almost a constant escape of offensive flatus per anum. This stage of the fit continues invariably for as nearly as possible two hours, or till eleven o'clock: and so certain have the friends become from their long experience, that they do not watch her till within five minutes of the expected termination of it. Within a second of the anticipated time the second stage of the paroxysm sets in, the first indication of which is a slight motion of one of the hands; instantly thereafter the patient must be raised to the sitting position, otherwise violent choking symptoms set in, and a rigidity of the whole frame, which, when allowed to have its full effect, is not afterwards easily overcome. She now seems to suffer violently, cries bitterly, with tears, though not copious. The respiration during this second stage is carried on by one long and rather crowing inspiration, and the expiration broken by three distinct hicks. I can find no word to express my meaning better. She invariably rests her head on the shoulder of some one, and in this posture seems to obtain most assistance. The blood-vessels of the neck, from the violence of her sufferings, are much distended. During this stage the discharge of flatus continues, but more interruptedly. This state of things lasts for three quarters of an hour, and punctually as that term expires, without any apparent diminution of the severity of the symptom, this second stage of the disease suddenly ceases, and she quietly lies down, seemingly but slightly fatigued, and disposed almost immediately to talk indifferently of any matter that may have come under her notice at any time previously to the setting in of these violent and extraordinary paroxysms. The second daily attack begins a little after twelve o'clock, and terminates somewhere about three; and the third and last attack comes on about four, and by seven o'clock in the evening her day's sufferings are over. She has never had an attack during the night, nor at any other time than at the periods mentioned above. For many months she has taken no solid food of any kind. After the last attack in the evening she may suck a small piece of orange, eat a piece of an apple, or the pulp of one or two grapes, and drink one or two tumblersful of water. Of these consists the whole nutriment she partakes of till the next evening, when the same diet is repeated. Her reasoning faculties and memory continue unimpaired, though a little childish in her manner, which may be easily accounted for by the indulgence of fond parents.

ART. 53.—*A Case in which Speech was Recovered after being Lost for several Years.*

By Dr. RAMSKILL.

(Medical Times and Gazette, December 27, 1862.)

CASE.—A girl, aged 22, from the country, was admitted into the Epileptic Hospital under the care of Dr. Ramskill, September 12, 1862. The following account is given nearly exactly as written by the patient's mother:—

“It came on with a bad cold and cough. Everything was tried that could be thought of to get rid of the cough, but it increased to a wonderful

curious noise, which kept upon her almost continually, so that there was but a very short time between for conversation. Before it came on again, she always complained of her chest, but could not describe how it felt. Her speech gradually left her, but about an hour in the day this noise would leave her, so that she could talk; from this time it got to half an hour, quarter of an hour, five minutes, and three minutes. After this the noise never left her except when sleeping, but as soon as she awoke it used to come on immediately, and so it continued for some time; a very low noise at times, then louder. After a time the noise was changed to very loud, but it used to cease several times in the course of the day. So it continued for some time. Then it used to come on only once in the day, and that was of a morning. At first waking, she used to shriek for about five or ten minutes; it could not be compared to anything else but a shrieking fit. This continued for three or four years every morning, and at last left her entirely. Not less than five different noises I have heard during her affliction. It is about five years since this noise left her entirely. Since she began to be regular, which was about the age of seventeen, she has gradually gained her speech so far as to be able to talk a little, but for the last three years there seemed not to have been any improvement. She cannot mention the name of any person or thing, if it be ever so easy, which is very remarkable; and cannot try to say a word, as children do when learning to talk; nor can she enter into conversation at all, unless persons ask her questions, and then she can answer in very simple words, but never can say a long word. She had her natural speech up to eight years of age, and was a very active child. I have never heard her read out since that time. She has had medical attendance several times. At one time it was thought that the spine of her back was affected: she lay on boards for several weeks, but was not benefited by it. It was then thought that the windpipe was affected, but nothing for this did her any good. She was electrified, but it did not produce any effect. She is very nervous at times, and very low-spirited; and if she walks fast, short of breath. She has not been regular for six months. She had never passed any worms."

When first seen by Dr. Ramskill, she could not say the name of any person or thing. She could utter one or two words. She was, however, fairly intelligent, but nervous, and her memory was bad. When she wanted anything, she made signs for it, pointing, and saying, "I want that." The first new word she was taught (by the nurse, who took great pains with her) was "good-night." Whilst being taught, the nurse said her heart would beat violently, and she seemed hysterical; and for this reason she was generally taught at night, after the other patients had gone to bed. The improvement was, on the whole, gradual, except one rather sudden step on November 26.

December 20.—She has now been well for several weeks, and can speak well. On one occasion, however, after having been annoyed, she did not speak for five days. During the first week or two of her stay in the hospital, she had attacks of coughing—a hard, barking, clattering cough. It was worse when she was excited and when her speech was worse. The medicines taken were small, repeated doses of blue pill and strychnine (gr. $\frac{1}{15}$).

This case, Dr. Ramskill said, might lately have been complicated with hysteria, but it could not have been altogether due to it, as it began when the patient was a child only eight years of age. It was, he thought, rather of reflex origin.

ART. 54.—*Loss of Sight from Decayed Teeth.*

By Mr. H. T. KEMPTON.

(The Dental Review, January, 1863.)

CASE.—The patient was a gentleman, aged forty-two, of a highly nervous temperament. At the time he first visited me he was suffering from pain extending over both sides of the head and face, incapacitating him from attending to his profession, and depriving him of rest at night. The sight of both eyes was affected, but more particularly that of the right. The patient had been under treatment since June last. On examining the mouth I found there was extensive inflammation on both sides of the lower jaw, owing to the presence of two decayed teeth. On the right side the first molar was considerably decayed and the pulp cavity exposed, so that on introducing a small instrument it gave rise to a violent paroxysm of pain. The tooth was somewhat loose in its socket, owing to the inflammation having extended to the periosteum. Subsequently, upon extracting this tooth I found a small sack filled with pus attached to the fangs. On the opposite side, the second molar was in much the same state, but the inflammation not so acute.

In the upper jaw all the teeth were sound.

In consequence of the pains about the head and face, I extracted the two decayed molar teeth in the lower jaw. This proceeding not only afforded relief to the patient's sufferings, but was afterwards attended by such a marked improvement in the power of vision, that there can be no reasonable doubt that the teeth had been the primary source of the eye affection. The history of the case will, however, be best understood from the following account given me by the patient himself:—

"It may be necessary to remark that for some months previous to becoming a sufferer from toothache, I had worked hard at mental employment, and my nervous system was in a very sensitive and excitable state. My lower limbs particularly felt as if they were *partially* paralysed, or rather I should say that when at all tired I was to some extent unable to control them.

"In May last I visited the North of England, and while there suffered severely from what I then believed to be *tic douloureux*, but now concluded to have been toothache. It commenced on the left side of the lower jaw, doubtless from the pressure of the wisdom tooth upon the decayed tooth lately extracted. The pain I endured was excruciating, and was not confined to the lower jaw, but spread entirely round the face, and was at times acute in the ears and about the temples.

"On returning to London, I suffered greatly from pains in the teeth on both sides of the lower jaw, but found that the right ear and temple were more affected thereby than those on the opposite side.

"At length, about October 14th, I suddenly found that the sight of my right eye was all but gone. Attributing this to cold, I, as I now believe, very improperly, applied a warm poultice overnight. On the following morning I found my sight by no means better, and after foolishly dallying for a week or more, was advised by a friend to go to the Ophthalmic Hospital, which I did, and explained to the surgeon, Mr. Jabez Hogg, that I had been suffering from toothache as I have previously described. He at once prescribed for me, and said that if I valued the sight of my eye, the offending tooth must come out. I accordingly went to a dentist to whom I had been recommended, and told him that I must leave him to judge which

tooth it was, as owing to the general pain in the teeth in the locality (the right side of the lower jaw) I really could not fix upon it. He selected the wisdom tooth, which Mr. Hogg subsequently told me was the wrong one, and pointed out the tooth afterwards extracted by yourself, as the source of my sufferings. Under the skilful treatment of Mr. Hogg, the sight of my eye has been and is gradually improving; but there are one or two points, which, although doubtless well understood by ophthalmic surgeons, have seemed to me very curious.

"I observed that while, by the improvement of the sight of the affected eye, I was enabled dimly to distinguish the furniture and windows of a room—the fire, which I should have thought would have been the clearest object of all, was quite invisible, and simply conveyed the impression of a black space. While this was the case, I could nevertheless faintly distinguish the smoke ascending the chimney. As the vision continued to improve, I was gradually enabled to see the light proceeding from the fire—although the absolute heated coals and flame were still invisible to me. These, together with gas-lights and the flames of candles, are now becoming distinguishable.

"I may further observe, that on closing the eye, while it was yet all but insensible to external objects, I could see various beautiful patterns consisting of numerous combinations of gold or orange-coloured carved lines on a dark ground.

"As the sight gradually improved, these patterns became replaced by stars of a like colour on a similar dark ground—and finally both patterns and stars disappeared."

ART. 55.—*A Case in which there was a Remarkable
Enlargement of the Nerves.*

By Dr. WALTER MOXON.

(*Guy's Hospital Reports*, 3rd Series, Vol. VIII. 1862.)

During the winter session of 1860-61, Dr. Moxon chanced to observe that the nerves in a subject which was then undergoing dissection were of a very unusually large size. It happened, unfortunately, that this subject had been allotted to the use of first-year students, who at the school of Guy's are instructed to dissect the muscles of the body without regard to the vessels and nerves; in this manner it came to pass that the dissection was already advanced before the extraordinary condition of the nervous system was noticed; the brain had in the meantime been removed and destroyed, and the opportunity for making many interesting observations had passed by. Enough was left to establish the case as one of the most extraordinary upon record, for when the remainder of the nervous system was surveyed, the nerves which composed it were found to be uniformly increased in size throughout their entire extent in such a way that they had nearly three times their usual diameter; they were all enlarged in just proportion to their proper size, and there were no irregular swellings or tumours of them, or connected with them at any part; the fasciculi of which they were composed were individually larger than usual, so that these nerves looked coarse in texture; when one of the larger of them was cut across, the great size

of the fasciculi was plainly seen; there was no increase of the interstitial neurilemma, but rather a proportional decrease, so that the coarse component fasciculi were more than ordinarily independent of, and moveable upon each other. The nerves were enlarged in this extraordinary manner as far as the naked eye could trace them, and were not easily broken, so that branches usually so small as to be very difficult of manipulation could with great facility be followed close up to their ultimate distribution. In the microscopic examination of the state of these nerves we obtained the valuable assistance of Dr. Gull, also of the late Professor Quekett, and of Mr. Bader; careful measurements of the size of the nerve-tubules were made, and always with the same result, showing that the average diameter of the ultimate fibrils was $\frac{1}{800}$ th to $\frac{1}{650}$ th inch, some as large as $\frac{1}{500}$ th inch were measured, and in repeated observations upon portions of the median nerve we did not find any smaller than $\frac{1}{1000}$ th, the main proportion being much larger. It is this enormous size of the elementary fibrils which renders the case so remarkable; if the great size of the nerve-trunks had depended upon an unusually large number of these elements, the case would have been worthy of note, but we should have had some analogy for it in the large muscles of an unusually muscular man, or even, more distantly, in the numerous blood-corpuscles of one who is plethoric, or in the multiplied adipose vesicles of a very fat person; as the case stands, however, we must wait for a parallel instance, until some observer shall find a subject whose red blood-corpuscles are $\frac{1}{1500}$ th inch in diameter, and capillaries universally varicose, or his muscular fibres $\frac{1}{150}$ th inch. It is well established that the size of the elementary nerve-tubules is generally constant in man and mammalia, being about $\frac{1}{2000}$ th to $\frac{1}{1500}$ th inch, rarely a little larger, often much smaller; we are disposed to think, however, that the number of distinct observations, in the way of comparative measurement, is not large, and that there may be considerable variations in the size of the tubules in different subjects. If this be true, this case will be an extreme one, but one of a natural class.

The subject whose peripheral nerves are described in this article was the body of a female who died in Guy's Hospital of a chest affection. None but negative information could be obtained regarding the phenomena of her nervous system during life.

ART. 56.—*Cases of Nervous Affections treated by Faradaization.*

By Dr. JULIUS ALTHAUS.

(Pamphlet: Trübner: London. 1863. Second Edition. Pp. 16.)

The cases related in this pamphlet illustrate very plainly the efficacy of a mode of treatment which is yet very far from being appreciated at its true value. They form a fit sequel to those related in Dr. Althaus' excellent "*Treatise on Medical Electricity, theoretical and practical.*"

(B) CONCERNING THE RESPIRATORY SYSTEM.

ART. 57.—*On the Treatment of Pneumonia, with the Results of 105 carefully recorded Cases.*

By Dr. J. HUGHES BENNETT, of Edinburgh.

(*Lancet*, August 16, 1862.)

Pneumonia, like most other inflammations, when acute, was formerly treated by so-called antiphlogistics—that is to say, blood-letting, purgatives, antimonials, low diet, and other methods of lowering the strength of the patient. It was about eighteen years ago, in consequence of investigating the pathology of inflammation, that Dr. Bennett began to doubt the propriety of such a treatment, and this for the following reasons:—

In the first place, the cause of the inflammation is an irritation of the textures, of the ultimate molecules, of the part; in consequence of which their vital power of selection is destroyed, and that of their attraction is increased. The removal of blood by venesection cannot alter this state of matters, neither can other lowering remedies. If the inflammation be superficial and limited, local bleeding may diminish the congestion, as in conjunctivitis; but if exudation has occurred, it cannot remove that.

In the second place, an exudation or true inflammation having occurred, it can only be absorbed by undergoing cell-transformation. Now this demands vital force or strength, and is arrested by weakness. Inflammations in healthy men rapidly go through their natural progress. In weak persons they are delayed or arrested: hence their fatality.

In the third place, the strong pulse, fever, and increased flow of blood in the neighbourhood of inflamed parts have been wrongly interpreted by practitioners. They are the results, and not the causes, of inflammation; and show that the economy is actively at work repairing the injury. So far, therefore, from being interfered with and interrupted, they should be supported by nutrients.

It follows, fourthly, that if these views be correct, the true treatment of inflammation, should be directed towards bringing the disease to a favourable conclusion by supporting, rather than by diminishing, the vital strength of the economy; and this, not by over-stimulation, as was done by Dr. Todd, but simply by attending to all those circumstances which restore the nutritive processes to a healthy condition.

In support of the soundness of these views, Dr. Bennett offers an analysis of 105 cases of pneumonia publicly treated in his clinical wards in the Royal Infirmary of Edinburgh, and carefully recorded by his various clinical clerks. In all these cases the treatment was directed to the support of the economy, never to weaken it by antiphlogistics. At the same time, if dyspnoea be urgent, cupping or a small bleeding (from four to eight ounces) may be practised as a palliative, more especially in bronchial or cardiac complication—although in none of these cases was such bleeding ever found neces-

sary by him. During the febrile excitement mild salines are administered. On the fourth or fifth days, when the fever abates, good beef-tea and nutrients are administered; and on the pulse becoming soft or weak, from four to eight ounces of wine daily. As the period of crisis approaches, slight diuretics are given to favour the excretory process. The results are—

Single uncomplicated cases, 58	Average duration, 13½ days.
Double " " 19	" " 20 "
Complicated cases . . . 17	" " 15½ "
Unsatisfactory cases (as to } duration) }	8
Deaths	3
105	

Ratio of deaths, 1 in 35 cases.

Average residence in the hospital of 77 uncomplicated cases of pneumonia (single and double), 22½ days.

It has been supposed that in consequence of this comparatively small number of cases occurring in so long a period as fourteen years the disease is rare in Edinburgh. It should therefore be explained that the clinical professors are on duty alternately, and that Dr. Bennett had never acted as physician to the infirmary more than one-half of the year, and in most cases only one-third of the year. It has also been imagined that pneumonia in Edinburgh is unusually slight and trivial, or that the disease in these cases was not extensive. But it is not so. Many cases, and especially the double ones, were very severe, with great dyspnoea and very urgent symptoms. Dr. Bennett has frequently pointed out the existence in these cases of the hard and strong pulse in vigorous young men, in whom, however, the most rapid recoveries were invariably observed. It should also be noted that these cases were in no way selected, but included not a few which were admitted *in extremis* by the resident clerk, and never seen by the physician; nor such as were partly treated by other physicians in the hospital, and for which treatment Dr. Bennett was not responsible.

From these facts Dr. Bennett concludes—

1. That simple pneumonia, if treated so as to support, instead of lower, the nutritive processes, so far from being a fatal disease, almost invariably ends in recovery.

2. That the cause of mortality in these cases is exhaustion, either before they come under medical supervision, or, as formerly practised, by a lowering treatment. Bleedings or other remedies that do not exhaust must be regarded as palliative rather than curative, and their influence has yet to be determined with exactitude.

3. That the same rule applies to all inflammations, the amount of danger being in direct ratio to the weakness of the system; the existence of complications in other viscera, or blood poisoning.

Dr. Bennett is of opinion that these important results are not the effect of chance; of empirical experiment; of a change in the nature of inflammation, or of the force of the pulse in man and

animals ; of an alteration of diet or of drink, or of nervous susceptibility ; nor of a change of type in disease : all of which have been supposed by some explanatory of facts which can no longer be denied. The more he considers this subject, the more is he convinced that it could only be justly attributed to the advance of medical science, and that it was a source of infinite satisfaction for medical men so to consider it. He thinks it strange that some minds would rather ascribe so manifest an improvement in the treatment of disease to hypothetical revolutions in nature which had no proof in their support, than to the increase of knowledge amongst the profession which was obvious to all. It seemed to him that if any one demanded in what way our advance in physiology and pathology had benefited the treatment of disease, no better proof of it could be found than in the diminished mortality that everywhere now accompanied attacks of acute inflammation.

ART. 58.—*On the Employment of Acetate of Lead in Pneumonia.*

By Dr. LEUDET, of Rouen.

(*Bulletin Général de Thérapeutique*, November 15, 1862; and *British Medical Journal*, January 17, 1863.)

Dr. Leudet gives the results of his experience in the treatment of pneumonia by acetate of lead. He has given this remedy in forty cases, of which thirty-seven recovered and three died. The mean age of the patients was $36\frac{1}{2}$ years. Of those who died, one was 56 and the other 57 years old ; the third, whose age was 37, had been previously treated by tartar emetic in large doses, and presented symptoms of inflammation of the mouth and pharynx. The mortality has thus been 7 per cent. Five of the patients were from 16 to 20 years old, twelve from 21 to 30, six from 31 to 40, six from 41 to 50, eight from 51 to 60, and two above 60. Of the whole number, thirty-one were males and nine females—a favourable circumstance, according to the statistics of Huss and others. The pneumonia was double in one case only. Of the thirty-nine unilateral cases, the base was affected in twenty-three cases, the apex in ten, the middle of the lung in one, and the entire lung in five cases. The amount of acetate of lead given in twenty-four hours varied from a grain and a half to twelve grains. The total quantity administered in the course of the disease varied in different subjects from seven grains and a half to eighty grains ; the average quantity was nearly two scruples. The duration of the use of the medicine varied from one to fifteen days, the average being six days. The medicine was always given in the form of pill. M. Leudet has not observed in his patients any signs of lead-poisoning, nor the “lead-line” on the gums, nor constipation. On the contrary, diarrhœa was observed in more than half of his cases, most of whom were taking large doses of the acetate of lead. He gives reasons for believing the diarrhœa to have been a result of the treatment, and not of epidemic influence or other causes.

When the treatment was commenced in the first four days of the disease, a cure was obtained, in thirteen cases out of thirty-seven, before the twelfth day; and when the medicine was first given between the fourth and the eighth days, the average date of cure was the fifteenth day. The effect on the pulse was to diminish the number of pulsations from 100 and upwards (to 120) down to 70, 60, and even 50 or 40, on the fourth day after commencing treatment. In most cases, M. Leudet has not found the pain in the side relieved by the acetate of lead, but has been obliged to employ local blood-letting. In twenty-seven cases, the period at which the sputa lost their rusty colour was observed. In twenty cases, the decoloration was complete from the sixth to the ninth day. In one-half of the cases, the returning crepitant rhonchus was heard after the first day of treatment. In nine cases out of thirty, the stethoscopic signs remained stationary for two or three days, when resolution manifested itself. In six cases out of thirty, the symptoms increased in intensity after the treatment was commenced; but this state continued one day only in five cases, and two days in another. Where once resolution has commenced, it is useless to continue the acetate of lead. In two cases, M. Leudet observed relapses, one of which occurred while the lead was still being given. Convalescence was very rapid; and the appetite returned as soon as the fever disappeared, even before the resolution of the disease was well established.

ART. 59.—*Some Remarks on Whooping-Cough.*

By Dr. HENRI ROGER.

(*Gaz. Hebd. de Méd. et Chir.*, February 18, 1863.)

In a clinical lecture on this subject, delivered at the Hôpital des Enfants Malades, in Paris, Dr. Roger expresses an opinion that the presence of fever in its incipient stage removes whooping-cough from the order of neurosis to that of eruptive fevers. He considers whooping-cough, in fact, as a specific disease, a kind of nervous bronchitis, of which the only appreciable cause is contagion. With respect to this point, he says:—"Of eleven cases at present in M. Roger's and in M. Blache's wards, six were evidently due to this agency. The fœtus in utero enjoys no immunity in this respect, and MM. Rilliet and Barthez relate the history of a child, whose mother, at the time of her accouchement, was suffering from the disease, and who, on the first day of extra-uterine life, betrayed unmistakeable symptoms of whooping-cough. M. Blache has also observed the affection in an infant aged ten days, who took it from its mother. If certain members of a large family escape contagion, the circumstance is referable to idiosyncrasy or to insufficiently prolonged contact; but a communication of a quarter of an hour's duration is enough for the transmission of the malady, and the poison preserves its morbid power for one and even two months, but seldom more. In fact, the pathogenic influence is chiefly marked during the catarrhal stage of whooping-cough, *i.e.*, during the first fortnight of

its existence, because at this period the true nature of the affection is still doubtful, and fewer precautions are taken to guard against contagion. But as all epidemics must perforce begin with one case, it is rational to admit the existence of spontaneous whooping-cough, as well as of spontaneous glanders and rabies."

ART. 60.—*On the Dimensions of the Chest in Tuberculosis.*

By M. HENRI GINTRAC, of Bordeaux.

(*Journal of Practical Medicine and Surgery*, November 23, 1862.)

In order to obtain a trustworthy criterion, M. Gintrac began his investigations by ascertaining the correct dimensions of the chest in 140 individuals in the enjoyment of sound health, and afterwards by measuring the thorax circularly in 80 cases of phthisis. He divides the subjects into three sections according to their age, and subdivides each class into two groups corresponding to the stages of tubercular consolidation and of softening. These researches have led him to the discovery of the fact that the interval between the mammæ conveys a correct notion of the average circumference of the chest.

M. Gintrac concludes with the following statements:—

1. In consumption, the circumference of the chest is smaller than in health.

2. The diminution increases with the progress of the disease, and in the second stage of tuberculosis may extend to four inches for the upper, to three inches and a quarter for the middle, and to two inches and a quarter for the lower part of the thorax.

3. At all periods of phthisis, the superior circumference of the thorax is more considerable than the mammary or inferior periphery.

4. The interval between the nipples, in the male subject, conveys an accurate idea of the general dimensions of the chest, and is equal to the quarter of the mammary circumference; in the adult, that measurement equals eight inches in health, seven inches and a half in the incipient stage of tuberculosis, and six inches and three-quarters in the more advanced periods of the affection.

5. The measurement of the intermammary space deserves the attention of the practitioner, and should be taken into account in the estimation of the tendencies to pulmonary consumption.

The author, therefore, remarks, that the propriety of special hygienic measures, and peculiar gymnastic exercises of the chest, is a necessary inference from the preceding observations. The thorax should be enlarged by graduated efforts of inspiration, and expanded by muscular exercise of the arms, in which abduction should predominate. In short, we should seek in the very act of respiration for the means of remedying an insufficient development which may prove injurious to the lungs.

ART. 61.—*On the Peculiar Shape of the Fingers in Chronic Diseases of the Chest.*

By M. TROUSSEAU.

(*Journ. de Méd. et Chir. Prat.*, November 16, 1862.)

M. Trousseau carefully points out every year to his pupils the peculiar deformation of the hand, called *hippocratic* or *clubbed finger*, which, since the most remote antiquity, has attracted the notice of pathologists. Ancient writers thus describe the appearance of the fingers in consumptive subjects:—*Tabidis unguēs contrahuntur*, or "*tabidis unguēs adunci*." In 1832, M. Pigeaux, a Paris practitioner, again invited attention to this, in some measure, forgotten symptom, and endeavoured to establish with precision its semeiotic value. M. Trousseau describes it as follows:—It is a shortening of the third or ungual phalanx, attended with inspissation and transversal enlargement of the digital extremity. The nail, at the same time, becomes incurvated, and the point of the finger assumes the shape of a club, or, more properly, of the head of a serpent. The deformation is sometimes a slow process, but occasionally takes place very rapidly, and not without pain. M. Trousseau remarks that, not having observed clubbed fingers in genuine scrofula, nor in uncomplicated abdominal tuberculosis, he inclines to consider the symptom as special to chronic affections of the chest. He has met with it in the second and third stages of pulmonary consumption, and in young subjects suffering from chronic pleurisy. The sign is not, therefore, exclusively pathognomonic of phthisis; but the clubbed finger is much more frequently observed during the progress of that affection, and the unciniform nail becomes better marked in proportion as the disease is in a more advanced stage. In the main, M. Trousseau opines that the hippocratic finger affords valuable presumptive evidence of the existence of pulmonary consumption. He ascribes the alteration of shape to hypertrophy of the bone, or at least to preternatural development of the fibro-cellular tissue in the pulp of the finger, invading the thumb and index, first of the right and afterwards of the left hand; the other fingers becoming successively affected in the order of their development, the little finger thus preserving sometimes its natural form, while all the others are already distinctly clubbed. M. Caron recently brought the question before one of the medical societies of Paris; and although he agrees with MM. Pigeaux and Trousseau as to the semeiotic import of the symptom, he connects it also with the scrofulous diathesis. In a recent number of the *Revue Médicale*, a new explanation of the deformation of the fingers is given by M. de Saint-Maclout. He observes, that in cyanosis, as M. Gintrac has before correctly remarked, the digital extremities sometimes assume the hippocratic character. The disturbance of nutrition in cyanosis is consequent on the admixture of venous with arterial blood; and it does not appear improbable that the same morbid confusion of the two kinds of blood also induces the clubbed form of the tips of the fingers in

phthisis. M. de Saint-Maclout adduces in support of his opinion M. Natalis Guillot's researches on the *disarterialisation* of the blood which passes through the lungs in tuberculosis.

ART. 62.—*On the Therapeutics of Consumption.*

By Dr. COTTON, Physician to the Hospital for Consumption at Brompton.

(*Lancet*, October 25, 1862.)

This paper embodies the results of some experiments which Dr. Cotton has been carrying on for the last five years at the Hospital for Consumption, Brompton, upon the action of certain medicinal substances upon phthisis. Dr. Cotton has exhibited the following substances, each in twenty-five cases of simple uncomplicated consumption—viz., phosphorus, liquor potassæ, hydrochloric acid, iodide of iron, iodide of potassium, chloride of sodium, vinum ferri, glycerine, sesquichloride of iron, chlorate of potash, quinine, and phosphoric acid. The phosphorus was administered in doses of about one-twenty-fifth of a grain two or three times a day; in a few cases it seemed to do good, but more generally its effect was unsatisfactory. Liquor potassæ was productive of very little good, and presented a marked contrast to the dilute hydrochloric acid, which evidently contributed in many instances to marked improvement in the patients; only 12 per cent. of those who had taken the potash having been benefited, whilst 68 per cent. of those who had taken the hydrochloric acid received more or less good. Amongst the latter were some very decided cases. Iodide of potassium contributed but few cases of improvement, its effects being generally of a negative character. Iodide of iron, however, made a very respectable addition to the list of improved cases. In many instances chloride of sodium acted favourably as a tonic, both the appetite and general power of the patients appearing to increase under its use. Steel wine was productive of very good results, especially in children and young persons. The author appears to place great faith in its use, particularly when given with, or immediately after, meals. Glycerine failed very generally, and its effects could bear no comparison with cod-liver oil, both these substances having been carefully weighed one with the other. Of all remedies, the sesquichloride of iron seemed the most effective, a considerable number of patients (66 per cent.) having derived more or less advantage from its use. Dr. Cotton states that both in his hospital and private practice he had long regarded this as one of the most useful agents in the treatment of the ordinary run of consumptive patients. Chlorate of potash proved useful in many of the very cachectic cases; but it was far from a "specific"—a title given to it by more than one medical practitioner. Quinine is less effective than many other tonics, although it did good in a small proportion of cases. Steel and quinine, however, are spoken of by the author in very laudatory terms. Phos-

phoric acid acted as a tonic in a certain number of cases, but is inferior to other mineral acids, especially the hydrochloric.

The following are amongst the conclusions which Dr. Cotton wishes to draw from the facts detailed :—

1st. That since, during the administration of each one of the agents he has experimented upon, several cases were observed to run through the various phases of the disease, some to a fatal termination, it is obvious that whatever the amount of benefit which in some cases followed their use, no one of such agents deserves the title of "specific."

2nd. It may fairly be concluded that the good effected by any of these agents was due to their respective tonic and upholding influence upon the general system.

3rd. In the majority of phthisical cases, steel—especially the sesquichloride of iron—and the mineral acids appear to be the most effective; but tonics generally are productive of more or less improvement.

4th. Since, however, even steel and mineral acids, as well as other useful tonics, are undoubtedly inert in a certain proportion of cases, it is not improbable that there are varieties or modifications of phthisis, each of which may require a particular treatment. As there are special varieties of many other diseases requiring special modes of treatment, and yielding to none other, it is possibly the same with consumption.

Dr. Cotton observes, in conclusion, that we are too apt to consider and to treat phthisis as a separate and always similar disease, forgetting the almost endless varieties or modifications it presents; and he believes that at no distant period of time consumption, like many other diseases, will be shown to present definite and special forms, each of which requires definite and special management.

ART. 63.—*On the Employment of Arsenic in Phthisis.*

By Dr. ARTHUR LEARED, Physician to the Royal Infirmary for Diseases of the Chest.

(*Medical Times and Gazette*, and *Dublin Medical Press*, January 28, 1863.)

Dr. Leared relates nine cases of hospital patients in which he gave arsenic conjoined with cod-liver oil and sedatives, which latter remedies had been in use in most of the cases previously. He gave the arsenic with a view to retard that retrograde metamorphosis of tissue which seems to be particularly active in phthisis. Nearly all the patients were in an advanced stage of disease, when the irritability of the stomach, as well as the general susceptibility of the system, were likely to interfere with the medicine. Four were obliged to discontinue it after very short trials; but it is probable that hæmoptysis, which was the cause of discontinuance in one instance, and occurred in two other cases, was simply a coincidence. Of the remaining four, three affirmed the cough or expectoration to

be improved, one that pains of the chest were removed, two that night-sweats had been arrested, one that flushing of the face was much relieved, and one that appetite was greatly increased. It seems from these cases that arsenic would be useful in phthisis by virtue of its action on the respiratory system, as well as its tonic properties, but that it is not easily borne by the digestive system, even when combined with sedatives. If anything is to be effected through the chemico-vital action spoken of, it will only be early in the disease, when the vital power is comparatively unimpaired and capable of repelling any injurious effect of the drug.

ART. 64.—*On the Action of the Hypophosphites of Soda and Lime in Cases of Phthisis.*

By Dr. COTTON.

(*Lancet*, April 25, 1863.)

Dr. Cotton has carried out the treatment recommended by Dr. Churchill, in twelve cases under his care in the Hospital for Consumption at Brompton, taking the precaution at the same time to obtain the medicine from the chemist in Paris who supplies it to Dr. Churchill. These cases, which are given in the paper, were selected on account of their presenting nearly every possible condition both of the disease and of the circumstances of treatment. Two were unpromising cases, but most of them were promising; some had already improved under treatment; in others, no previous remedies seemed to have had much effect. The result was unsatisfactory, as will appear in the following quotation:—"Now, as I failed to observe any such consequences as Dr. Churchill describes in any one of my patients within two, or at most three, weeks of their taking the hypophosphites, I considered that it would be unjustifiably trifling with the disease to give a longer trial to the treatment."

ART. 65.—*A Résumé of Twelve Years' Experience of Paracentesis Thoracis.*

By Dr. HENRY I. BOWDITCH.

(*American Journal of Medical Sciences*, January, 1863.)

This paper, which was read before the Boston Society for Medical Observation, gives a *résumé* of the author's large experience in this matter.

Between April 17th, 1850, and Dec. 17th, 1860, Dr. Bowditch has operated 150 times on 75 persons, and he has seen the operation performed in ten other cases, making 160 operations on 85 persons. The plan adopted in the majority of cases was that suggested by Dr. Wyman—namely, the exploring trocar and a suction-pump.

Innocuousness of the operation.—Dr. Bowditch has never seen the least permanent evil resulting from any operation, and *only the slightest temporary* difficulties, such as pain, slight dyspnoea, stricture, or cough, &c.

Frequency of the operation.—One lady, at the first operation $4\frac{1}{2}$ months pregnant, and in whom the orthopnoea was several times so great, that death, it was apprehended, would have supervened within twenty-four hours, if the operation had not been performed, was tapped nine times in $8\frac{1}{2}$ months. She is now tolerably well, but with a contracted chest, as is usual in chronic pleurisy.

In striking contrast with this case, as it regards the *frequency* of the operation, while resembling it in the number of times it was performed, was the unique case of an elderly man very recently under treatment, and in which the tapping was practised eight times in six weeks! The patient, himself a physician, earnestly and even solemnly demanded the operation as a mere means of relief to intense distress. As he jocosely remarked on one occasion, he considered it one of his "luxuries."

Cases in which the patient recovered wholly.—Twenty-nine out of the seventy-five patients got wholly well, apparently in consequence, chiefly, of the operation. The operation was performed generally when severe symptoms were manifest. In a few, a great quantity of fluid was recognised by the physical signs alone, the rational having been slight; but as the disease was chronic, an operation was deemed necessary. In all these cases, the operations seemed the *first step* towards a cure.

The character of the fluid and its influence on the prognosis.—In 26 out of the 75 serum was found; and 21 of these patients got wholly well. If after the first operation the fluid becomes purulent, an almost certain fatal prognosis should be made. The author has seen six such cases. Four of the patients died, two were lost sight of, but, when last seen, were failing.

Pus was found at the *first* operation in twenty-four cases. Once it was of the consistence of honey, but it was easily drawn through the exploring tube. Seven of these patients recovered wholly; seven died; nine were relieved one or many times; but they had either a long and tedious illness, terminating usually in phthisis, or a fistulous opening, or a still doubtful result.

A *sanguinolent fluid* at the first puncture (that is, a dark red thin fluid, evidently stained with blood, though not coagulating) Dr. Bowditch considers almost certainly fatal, and a consequence of some malignant disease of the lung or pleura.

There were seven of such cases. In six the patients died. In one there was a doubtful result, but apparently fatal tendencies were commencing. If the fluid is found sanguinolent at the second or any subsequent puncture, it is regarded of comparatively *little* importance towards the prognosis.

A mixture of *bloody purulent fluid* at the first operation is usually fatal. Three cases, all fatal, occurred.

A *fetid gangrenous fluid* is very rare, only one case having occurred, and that fatal; but in this case infinite relief from horrible

orthopnoea was obtained, and it never returned, though the patient sunk and died in a few days. Gangrenous pleura was found.

Dr. Bowditch has operated once in *pneumo-hydrothorax* with temporary relief and comparative ease for several days, and he says that, as the operation can do no harm and may give much relief, he is prepared to operate again in any case where the dyspnoea may be so great as to require it.

Cases where no fluid is obtained.—In seven cases no fluid was obtained. These cases occurred usually in the earlier operations, and the failure was often owing, as it would seem, to the cautious and slow manner with which the trocar was plunged between the ribs, carrying thus the false membrane of the pleura costalis before the instrument instead of piercing it; so that a valve was really formed over the end of the canula. At other times there is little doubt that an error of diagnosis was made, and that instead of a fluid there was simply an unexpanded lung and thick false membranes on the pleura, causing as much dulness on percussion and absence of respiration as a fluid would have done. The differential diagnosis of the two was not, at first, quite so easy as it is now. Inspection usually is the test between the two conditions; the intercostals being distinct and depressed when a membrane exists; but very indistinct and level with the ribs, or possibly prominent, when a fluid occupies the chest.

Once an immense tumour occupied and uniformly distended one pleural cavity, and in its course presented all the phenomena, natural and physical, of simple pleurisy. Dr. Bowditch tapped three times, viz., at the back, side, and front, at the same visit. No evil followed.

When shall we operate?—The author operates in every case, however recent or chronic may be the attack, provided there is permanent or occasional dyspnoea of a severe character, evidently due to the fluid. He has, of course, more hope of doing good where the disease has not been of too long duration, is uncomplicated with phthisis or any other disease, and where, moreover, the amount of fluid seems directly the cause of the trouble. He also deems it best to operate in *any*, even latent cases, where the pleural cavity gets full of fluid, and if, after a reasonable amount of treatment, the fluid does not diminish.

Where shall we operate?—The point originally chosen by Dr. Wyman and the author, viz., in a line let fall from the lower angle of the scapula, and between the ninth and tenth ribs, is the most appropriate point at which to make a puncture. Dr. Bowditch, however, has tapped under the axilla, or in the breast, where the case seemed to require it. In selecting the precise intercostal space on the back, he usually chooses one about an inch and a half higher than the line, on a level with the lowest point at which respiratory murmur can be heard in the healthy lung of the other pleural cavity.

He never waits until *pointing* commences; for then he is sure that pus will be found. If *pointing* without opening has commenced, he does not necessarily tap in that place, as recommended by the older surgeons, but seeks the most depending point in the chest. While

thus desiring to operate before a *local* distension shows itself, he dislikes or refuses to tap where there is contraction of the intercostal muscles; and he is certain of getting fluid only where there is distension or flattening of the same.

Objections to the operation.—After mentioning several objections, Dr. Bowditch says:—

“One and all of these objections are to me, with the experience I have had, simply absurd. Let any man have good instruments and manage them skilfully on proper cases, and he will agree to the truth of what I state.

“The operation, like everything else in all the departments of human life, is imperfect. It cannot cure all. But it has relieved many, and will continue to do so, if surgeons will use it; it has been the prominent cause of relief in many more, and will be so hereafter, if men will theorize less and act more. It has been the sole means of saving life, I am sure, in a few of my cases; and I know some patients have died within the last few years, in New England, as I believe, for want of it, under the care of others.

“It is certainly innocuous, and gives so little pain, compared with the relief it affords, that patients have begged for it to be repeated again and again, as a mere matter of relief. In my opinion it ought never again to be allowed to fall into disuse by the profession. I regard any man who allows a patient to die of dyspnoea from pleuritic effusion, however great may be the complications with other diseases of head, chest, or abdomen, as in the dilemma of him who is either wilfully neglectful of some of the means of relief or cure, now by experience proved to be always at hand, or ignorant of the simple and beautiful operation suggested by Dr. Wyman.

* * * * *

“The result is the experience which I have given above. And now, as I have often said, I would as readily puncture the chest as I would draw a tooth, or vaccinate a child.”

ART. 66.—*On the Use of Perchloride of Iron and other Chalybeate Salts in the Treatment of Consumption; being a Clinical Inquiry into their Physiological Action and Therapeutical Properties; with a Chapter on Hygiene.*

By Dr. JAMES JONES, Physician to the Infirmary for Consumption, &c., Margaret-street.

(12mo. pp. 109. London: Churchill. 1862.)

The object of this small volume is shown in the title: the manner in which this object is carried out is sufficiently satisfactory. In our opinion, indeed, the author does good service to practical medicine in recalling attention to facts which were more familiar before the introduction of cod-liver oil as a medicine in consumption, and in showing that steel has claims to notice in this case, to which even the claims of cod-liver oil itself may not be superior.

"From the study of the influence of perchloride of iron in many such cases," says Dr. Jones, "I have deduced what to my mind is a most convincing proof of its power in arresting the development of tubercle, and in altering the constitutional condition which precedes and attends its development.

"It is only by the experimental use of a remedy, in a large number of cases, that evidence of its power can, with any degree of certainty, be obtained; but results thus deduced possess a high degree of trustworthiness. It is on such grounds that I would advocate the use of preparations of iron, but especially the perchloride, in the treatment of consumption. I have taken advantage of the opportunities afforded me, as one of the physicians to an infirmary for consumption, and to a general hospital at which the attendance of out-patients is very large, to subject the remedy to the test of practical experience. I have used it in all stages of the disease, and under every variety of circumstances, and have carefully watched its effects from week to week. The treatment has extended over periods varying from a few weeks to eighteen months or longer. I have given it alone, and in conjunction with cod-liver oil. The results of this extensive and protracted trial of its powers have convinced me that iron, but especially the perchloride, possesses highly remedial virtues, and merits great confidence in the treatment of every stage of phthisis. In the early stages of the disease, but more especially in the condition of pre-tubercular dyscrasia, I have found it to be capable of establishing a state of perfect health. In the more advanced stages, even where cavities existed, I have seen numerous recoveries take place under its use; the patients being able to resume their business engagements and occupations, from the performance of which they had long been disabled. Even in the very advanced and hopeless stages, the distressing night-sweats and the sense of exhaustion and sinking have been to some extent relieved by its use. The whole number of cases treated on this plan amounted to about five hundred."

(c) CONCERNING THE CIRCULATORY SYSTEM.

ART. 67.—*On the Significance of the Frequent Pulse.*

By Dr. LATHAM.

(*British Medical Journal*, January 17, 1863.)

"The nervous system," says Dr. Latham, "has the heart for its gnomon or finger of the clock. This notes, by the rate of its movement, the various degrees in which the nervous system is affected, from the least to the greatest. It is the greatest which must now engage our attention, and the rapid pulse the index of them. And the cases which have been just cited will help to explain them. To press the language of our analogy a little further, for the sake of illustration, we have already seen the index hurrying rapidly round the dial-plate, and telling that, from some cause or other, the mechanism within was running down, and, if it were not arrested, that it would quickly stop. Even prior to any outward presentments to give assurance

of disease, even earlier than its known beginning, we have seen the countless fluttering of the heart and arteries give token of the nervous system already under trial of mortal suffering, and ready to let life go for ever. But there is no reason in the nature of things why a morbid poison, or any other element of disease, should not affect the nervous system soonest of all, and sooner than the blood and blood-vessels, to which it properly belongs to work out disease into its cognizable products and realities. Only experience says that it is rare, very rare.

"Not until disease is shaped and realized into something which can be seen or heard or touched, and can be called by a name, is it wont to engage the nervous system perilously; and should the particular disease, according to its nature, have to go through a stated course to a certain end, and so admit a probable measure of its duration, it will not commonly be until towards its decline that extreme peril to life is indexed by extreme frequency of the pulse. But the time is often anticipated. This sign of fearful significance—this countless fluttering of the pulse—may break in upon the disease at any stage of its progress, either early, even very early, or in its midcourse. And when it does, some notable thing has often preceded it, well capable of giving a shock to the nervous system. Sometimes a sudden profuse diarrhœa has been enough to account for it; sometimes a hæmorrhage; sometimes an unhappy error of treatment by larger depletion than the patient could bear; sometimes pain—severe, long, uninterrupted pain; sometimes loss of sleep; sometimes the solution of some organic texture within, as the rupture and perforation of a bowel. Sometimes, however, and oftenest, nothing incidental or casual can be found to bear the blame, but only the malignant nature of the disease itself.

"Still there is a hope for these cases, but it is small; yet small hope is made much of by the hopeful, and often serves them for a great encouragement. And so it must and had need do now, or there will be no saving of life.

"And there is a treatment of these cases. Death is inevitable, if they are left to themselves. None ever recovered but by known, palpable, and adequate means. The treatment is simple enough. A single symptom has absorbed all other indications of treatment into itself, and remains our sole infallible guide what to do. The countless fluttering pulse now stands for everything. Watch must be kept upon it and its meaning; and its meaning is, that the nervous system is ready to sink into death. Accordingly, stimulus is to be given so *often* and so *much* as will keep it going and prevent it from coming to a stop. All this is simple enough in the telling. I wish it were so in the doing. As much, perhaps, has been told as can be told. But the 'how often' and the 'how much' are the things upon which life now hangs, and a judgment has to be exercised upon them every hour or many times every hour. Therefore the sick man has a poor chance, unless he can now have the services of one intelligent nurse at least entirely to himself. To have also one intelligent physician constantly within reach would not be at all beyond his needs. But it is useless to insist upon what is generally impracti-

cable. Nevertheless, many cases within my experience, in which life has been rescued contrary to all probable calculation, have had this unspeakable advantage; and some of them have occurred in the families of physicians.

"But be it borne in mind that we have now to do with a part only of a very great subject. The nervous system, and its actings and sufferings under disease, this is the great subject; and our small part of it is the frequent pulse. Nevertheless, small though it be, it is the part which especially offers itself as a handle to the whole—the part by which we can best lay hold of it, and apprehend it, and study it, and turn our little knowledge of it to practical uses and benefit.

"It is only in the most chronic diseases that the nervous system does not sensibly participate. All which show an appreciable progress from day to day—all to which fever in any measure belongs, either essentially or as an accident; these cannot exist without the nervous system feeling them, and showing that it feels them. Now, almost all such diseases have, in respect of their essence, a due course to run and an end to reach. Phlebitis has its mixture of pus with the blood, and its deposit in abscesses within and without; erysipelas has its diffused redness and swelling; scarlatina one form of rash, and measles another; small-pox has its pustules, typhus has its petechial maculæ of the skin, and the typhoid malady has its tumescence and ulcers and sloughs of the intestinal glands. And these several conditions represent the essence of the several diseases, and show it to be different in each. But there are other things belonging to them all in common, which have great force in guiding their treatment and ruling all concerning them. It is, indeed, by the essential element of these diseases, and by its mode of working, whether diffused through the veins, or collected in abscesses, or sprinkled over the skin, or scattered through the bowels, that each fulfils its course and accomplishes its end. But it is the great vascular system and the great nervous system that are most conspicuously engaged, and acting and suffering all the time and in all of them, and the sum of their acting and suffering is called fever. The number of the pulse is our chief handle for apprehending the part specially borne by the nervous system.

"If the fever be small, with moderate sympathy of the nervous system, there will be moderate frequency of the pulse, with moderate increase of its power; and thus much is natural and perhaps needful. If the fever be greater, with more sympathy of the nervous system, the pulse will rise both in frequency and force. And still all is natural, and all is in harmony.

"But as the sympathy of the nervous system continues to increase, and the frequency of the pulse to increase along with it, anxiety begins to arise; yet the strength of the pulse will still be the vital safeguard. Let, however, the sympathy of the nervous system increase more and more, and the number of the pulse run on to greater and greater frequency, then assuredly its strength will languish; and if the balance be not soon redressed, it will become a countless fluttering. And to such extremity is the man now reduced, that this countless fluttering of the pulse will represent all

that is vital in him, and all that can be treated in the disease. Gradually or abruptly, in any of the diseases mentioned, this may become the condition of the patient. It is often witnessed in the typhus and typhoid fevers of late years.

"This deadly sinking of the nervous system, represented by the countless fluttering of the pulse, this most terrible incident of febrile diseases, coming on at their beginning, or middle, or end, sometimes abruptly, sometimes gradually, not absolutely hopeless, and not absolutely beyond reach of a remedy, deserves to have all told of it that observation has learnt. Well, then, having once begun, it may continue many days or nights (three or four) uninterruptedly; and during all that time the pulse will continue countless and almost imperceptible, abating nothing of its frequency, and gaining nothing of power for all the wine and spirits administered, but only just kept going and prevented from coming to a standstill. And thus the quantity even of spirits given in twenty-four hours has amounted to pints. And after these three or four fearful days and nights, the pulse may become of a calculable number again and of an appreciable power, and maintain itself at the less number and the greater power with a less amount of stimulus, and henceforth the disease will pursue its course exempt from the alarm of this particular symptom, which is the sure expression of the nervous system collapsing into death. But that the pulse should go on fluttering, and the nervous system go on collapsing into actual death, is the more common event.

"Or this terrible incident of febrile diseases may occur after a different manner. The pulse will be countless and fluttering for a few hours, and then, under the use of stimulus, it will recover power and a calculable number for a few hours, and then become countless and fluttering again. And after the nervous system has thus gone on to sink and to rally alternately for a few days, it will rally permanently, and the pulse become slower and steadier, and the disease will proceed free from this peculiar peril. But more often the nervous system will sink irrecoverably, and the pulse flutter and flutter on, and the patient die.

"There is a very curious and interesting circumstance, well worth our notice, to which my observation, if it do not deceive me, has often borne witness as a matter of fact. It is that the nervous system having reached the degree of collapse, indexed by the countless fluttering pulse, carries with it the pathological consequence of suspending for the time the course of the disease. Whatever be the disease which it attends, whether scarlatina, or measles, or erysipelas, or small-pox, and whatever be the stage it has reached, at that stage it will stop, and all specific morbid actions come to a suspense and standstill; perhaps to be resumed, perhaps not. To be resumed, when (if ever) the pulse comes within some clearly calculable number, and remains steadily within it, notifying the steady recovery of the nervous system. Not to be resumed, if the pulse goes on fluttering, although many days have yet to pass before the nervous system has collapsed into death. It is curious how disease thus under the all-subduing constraint of the nervous system, and halting for one, two, three, or four days, will, when that constraint is taken off, take up

its course again from the stage at which it was arrested, and duly proceed to its end."

ART. 68.—*A Case of Pneumo-Pericardium.*

By Dr. J. W. BEGBIE, Physician to the Royal Infirmary,
Edinburgh.

(*Edinburgh Medical Journal*, October, 1862.)

This case is one of malignant disease of the œsophagus, succeeded, first, by sudden pericarditis, and ultimately by pleuro-pneumonia with effusion.

CASE.—MRS. W., æt. 43, mother of seven children, admitted to Ward XIII., 29th July, 1862. She had for several months previously been under the care of Dr. Hislop of North Berwick, from whom, at the time of her admission, I received the following brief account:—"She had been suffering from increasing difficulty in swallowing, at first considered to arise from spasm in the muscles of the œsophagus, an opinion which was strengthened by the relief she experienced after passing the probang on several occasions. A month or more ago, in attempting to pass the probang much greater difficulty was experienced, and its use was finally desisted from. She suffered much about the same time from vomiting, and once brought up some blood with mucus." Dr. Hislop added, "From the pain she feels in the back, the increasing difficulty in deglutition, and the general features of the case, I fear that the morbid deposit is of a malignant character. I have for some time been doing nothing but supporting the system."

State on Admission.—Patient presents an anxious expression of countenance, is very anæmic, without history of hæmorrhage or renal disease. No albuminuria. As far as can be determined, the only cause for her present condition is defective alimentation, on account of dysphagia, which, coming on gradually, has existed more or less for nearly two years. She has almost constant vomiting, or rather there occurs immediate rejection of the food before it has reached the stomach. Has little or no pain. On careful examination of the chest, no abnormal indication is furnished either by the lungs or heart. Abdominal organs apparently free from disease.

From the time of admission the opinion gradually gained weight that the patient laboured under malignant disease of the lower portion of the œsophagus.

22nd August.—Under a careful regulation of diet some improvement has resulted. The dysphagia and vomiting have greatly abated. *Vespere.*—Has this evening complaint of headache and pain in the chest.

23rd.—After the application of a sinapism the pain in the chest was relieved. On auscultation, a distinct to-and-fro pericardial friction sound is audible over the region of the heart. There is no increase of precordial dulness. In the evening the patient fainted, losing consciousness for a very brief period; but on her recovery from the swoon, remaining cold and collapsed in appearance, with almost imperceptible pulse. Brandy was administered, and warmth applied externally.

24th.—Remained very much sunk during the whole night; the surface of body covered with clammy moisture; at times becoming almost pulseless; when perceptible, the pulsations at wrist numbered 120. Brandy and aromatic spirit of ammonia were given freely. She is now—*Noon*—a little

stronger, free from pain and without dyspnoea. The friction sound over the heart has lost nothing of its distinctness.

25th.—Has continued in much the same state. The attrition sound with the heart is not quite so distinct, and now there exists a little increase of dulness on percussion, with appearance of slight fulness in the fourth and fifth left intercostal spaces near the sternum.

26th.—More sunk in appearance. Physical signs have undergone no change.

27th and 28th.—In much the same state.

29th.—On auscultation to-day at visit, a very remarkable character of the heart's sounds was noticed. The friction is replaced by a gurgling noise, a *churning splash*, audible over the whole cardiac region, and rendered more distinct when, for an instant, the patient holds her breath. This sound is not distinguishable at a distance from the chest. The dulness on percussion over the heart has vanished, and now a clear and nearly tympanic note prevails, with increased fulness in precordial region. The patient's extreme weakness forbids any attempt to alter her position in bed; the effect of change of posture on the percussion note cannot therefore be determined.

30th.—Physical signs remain as yesterday.

31st.—Patient died at 9 A.M.

In endeavouring to explain the remarkable physical phenomena connected with the heart, which presented themselves during the closing days of this poor woman's life, I considered it probable that the pericarditis, of which, on the 23rd of August, the signs were perfectly distinct, was determined by the progress of the cancerous affection of the œsophagus to the posterior wall of the pericardium; and when, on the 29th, the friction sound over the heart was replaced by the gurgling rôle, limited to the cardiac region, and altogether unlike any sound connected with the heart's action previously familiar to me; and when, in addition to the evidence thus afforded, there had occurred an unmistakable alteration in the percussion note over the heart, dulness having yielded to clearness, I concluded that perforation of the œsophagus had taken place, and that, besides the presence of lymph and fluid in the pericardial sac, there was also air. The diagnosis then formed and expressed was as follows:—Cancer, affecting the lower portion of the œsophagus where in contact with the pericardium; pericarditis with effusion from extension of disease in the former; finally, rupture of the œsophagus and passage of gas into the pericardium. The *post-mortem* examination, conducted on 1st September by Dr. Haldane, determined the correctness of this opinion in all essential particulars. I subjoin Dr. Haldane's report.

"The body was much emaciated; the surface very pale.

"When the chest was opened, the pericardium, marked by the pressure of the ribs, bulged forwards, and on being punctured air escaped. There were no adhesions of the pericardium, but in its cavity were about three ounces of a dark-brown fetid fluid. Both surfaces of the serous membrane were coated with lymph of a yellowish grey colour, of leathery appearance, and evidently of some standing; there was also some softer and more recent lymph, which could be readily scraped off with the nail. When the heart, which was of natural size and structure, was removed, an irregularly circular opening admitting the point of the finger, and communicating with the œsophagus, was found in the posterior wall of the pericardium. On examining the œsophagus, its upper part was found healthy, but the whole of the lower part from about the middle of the thoracic portion was in a cancerous condition; about two inches and a half of its anterior wall was completely gone, and its cavity was here bounded by the back of the pericardium and by the inner margin of each lung. It was here that the

pericardium was perforated, and the pleura covering the lungs in this situation was dull and of a brownish colour, but the lungs were not opened into.

"While the liver was being removed, it was found that the back of its left lobe was adherent to the anterior wall of the stomach in a space about the size of half-a-crown. On separating the adhesions, an opening with sloughy margins was found in the stomach, but the firm connexion with the liver had prevented communication with the peritoneum. The whole of the lower part of the œsophagus, the cardiac extremity of the stomach, and the adjoining portion of its anterior wall were cancerous; the cancer was soft and fungating, and in several situations was in a sloughy condition. The intestines were contracted. There was no other lesion."

Dr. Begbie adds a few remarks on the physical signs of pneumo-hydro-pericarditis. "Laënnec," he says, "who probably exaggerated the frequency of the occurrence of gas in the pericardial sac, speaks of three signs upon which dependence is to be placed in the diagnosis of air and fluid in the pericardium:—1. Unusual resonance over the lower part of the sternum. 2. Fluctuation sound (*bruit de fluctuation*) audible with the action of the heart and on deep inspiration. 3. As specially relating to the diagnosis of pneumo-pericardium, the circumstance of the heart's sounds being heard at a distance from the chest. Upon this sign Laënnec placed very considerable reliance. He states, indeed, that his observations respecting it were made some time after those already referred to as *one*, and *two*, and that he had not been able to determine whether it existed in connexion with these. Dr. Stokes, whose observations on pneumo-pericarditis are most instructive, noticed the fact of the heart's sounds being heard at a distance in the case which he has recorded. He remarks, however, that this sign was not present in either Dr. Graves' or Dr. M'Dowel's cases already noticed. I have mentioned that it did not occur in the instance now recorded, and Dr. Walshe has no doubt correctly observed that Laënnec's expressed conviction, that in almost all cases (for he uses the expression *presque tous les cas*, and not simply *occasionally*) when the heart's action is heard at a distance from the body, the cause of the phenomenon is a temporary development of gas in the pericardium (often readily absorbed, and whose presence does not give rise to any serious result), cannot at the present day be received. In the remarkable case of pneumo-pericarditis related by Dr. Stokes, the following signs were observed. I give them in Dr. Stokes' own language. 'On examination a series of sounds was observable which I had never before met with. It is difficult or impossible to convey in words any idea of the extraordinary phenomena thus presented. They were not the rasping sounds of indurated lymph or the leather creak of Collin, nor those proceeding from pericarditic with valvular murmurs, but a mixture of the various attrition murmurs with a large crepitating and a gurgling sound, while to all these phenomena was added a distinct metallic character. In the whole of my experience I never met so extraordinary a combination of sounds. The stomach was not distended by air, and the lung and pleura were unaffected, but the region of the heart gave a tympanitic *bruit de pot fêlé* on

percussion, and I could form no conclusion but that the pericardium contained air in addition to an effusion of serum and coagulable lymph.' The phenomena on auscultation and percussion thus recorded will receive farther value as indicating the existence of hydro-pneumo-pericarditis, if in addition there be noticed, as was done by Dr. Walshe in the 'singular case of traumatic communication between the œsophagus and pericardium,' referred to in his work on *Diseases of the Heart*, a dull or tympanitic sound elicited over the precordial region according to the position assumed by the patient. The extreme weakness of the patient in the instance I have recorded alone prevented our determination of the existence of this important sign: from the appearances presented after death, I have little doubt that, had it been in our power to alter the patient's position after the development of the peculiar auscultatory phenomena, we should have had this last indication also to guide us. Without it, however, and in default of a metallic character of the cardiac sounds, as noticed by Dr. Stokes, the diagnosis of pneumo-pericarditis with effusion may I think be made, from observing a guggling or churning splash sound with the heart's action limited to the cardiac region, with which more or less of tympanitic precordial resonance is associated. Still more reliable as signs will these phenomena be, if, as in the instance now recorded, the guggling has succeeded, after its continuance for a few days, a distinct friction sound, and the tympanitic replaced a dull percussion note."

ART. 69.—*On Narrowing of the Aorta at the Point of Entrance of Botalli's Duct.*

By M. A. DUCHEK.

(*Wochenblatt d. Zeitschr. d. k. k. Gesellsch. d. Aerzte in Wien*, Sept. 10, 17, 24, 1862; and *Medico-Chir. Review*, Jan. 1863.)

At the conclusion of this interesting paper, one in which the literature, as well former as recent, is well worked up (no less than 51 recorded cases being referred to), the author has the following observations regarding the causes of death in cases of this kind. He observes that they may be well considered with reference to the age at which death occurred; with the exception of 9 cases, the details of which were not complete, and 3 cases which were still alive.

Of 39 cases, those that died—

Before the close of the 1st year of life, were 3 in number.

Between the 1st — 10th	"	2	"
" 11th — 20th	"	2	"
" 21st — 30th	"	10	"
" 31st — 40th	"	9	"
" 41st — 50th	"	6	"
" 51st — 60th	"	6	"
" 61st — 70th	"	1	"
In the 92nd	"	1	"

Exclusive of 11 cases, the particulars of which are deficient, the causes of death are indicated in 40 cases.

Firstly. In the first year of infancy, consequently soon after or during the process of the narrowing. The author remarks that the rarity of death at this period is not what might have been expected. In no case of this kind was there mention made of any enlargement of collateral vessels, and thus, he observes, death may be attributed to the rapid production of impediment. Even in the two cases narrated, where pneumonia or atelectasis of the lungs existed, the backward action of the obstruction on the smaller circulation of the lungs must be looked upon as having been concerned. The third case (that of a new-born child) cannot be considered one in which the cause of death was laid bare, inasmuch as nothing is declared respecting other organs.

Secondly. All the remaining 38 died after a longer continuation of the stenosis, the majority between the ages of 20 and 30, only a few between 30 and 60.

(a) Lacerations of the heart and large vessels occurred in 8 cases. Thus we have laceration of the right auricle in a man 35 years of age; of the right ventricle in a man of 57 years of age; of the ascending aorta in a patient 17 years old, in a man 23 years old, and in a man of "middle age;" of an aneurysm of the ascending aorta in a man of 21 and 24 years of age; of an aneurysm of the descending aorta in a patient 37 years old.

The author connects the stenosis and the ruptured heart and vessels, which so often occur, by supposing that a prematurely atheromatous and fatty condition is induced by the increased blood-pressure in the rear of the obstruction.

(b) In cases of a longer continuation of the disease, that is, in older men, dropsy and marasmus came on in 8 cases.

(c) In 3 cases death occurred quite suddenly, without any pre-existing symptoms; attributed by the author to irregularity and confusion, so to say, of the circulation suddenly (as by muscular exertion, mental agitation) induced; no opportunity, owing to the obstruction, being allowed for compensation.

(d) In 16 cases, in which the disease causing death had apparently no connexion with the stenosis, it was noticeable that they all had relation to the organs of respiration, with the exception of 3 cases—1 of jaundice after gall-stones, and 2 of tuberculosis. Thus, in 9 cases, pneumonia existed in patients of various ages, the youngest being twenty-two days, and the oldest fifty-seven years. In one case pleurisy, and in one atelectasis of the lungs existed. The author connects these conditions and their final result with the abnormal state of pressure in the smaller circulation which existed.

(e) Only one patient died from causes quite apart from the stenosis—viz., from marasmus senilis, at the advanced age of ninety-two years.

The general and final inferences drawn by the author are, that the stenosis of the aorta at the point of junction of Botalli's duct is almost always followed by important results; that these results are analogous to those which for the most part followed stenosis of the

vessels; that they are less rapidly fatal than stenosis of the cardiac orifices, but that this conclusion occurs in the majority of cases as the result of circumstances dependent on the stenosis of the aorta.

ART. 70.—*On Epidemic Angina Pectoris.*

By M. GÉLINEAU.

(*Gazette des Hôpitaux*, Nos. 114, 117, 120, 1862; and *Schmidt's Jahrbücher*, No. 1, 1863.)

This epidemic, which seems to be not unlike one which was described by Dr. Von Kleefeld as occurring at Dantzic in 1824, prevailed on board the *Embussade*, a corvette in the French Imperial navy, in 1858. The crew of this vessel numbered 250 men and boys. For some months this crew had been exposed to considerable alternations of climate, in consequence of the corvette being continually on the move between stations on the Mexican, the Chilian, and the Californian coast; and besides this, there was reason to believe that not a few of the number were abandoned to various most demoralizing practices. The epidemic angina pectoris, which broke out in the neighbourhood of St. Helena, after some days of very stormy weather, was preceded by a few isolated cases of the same disorder, and of colic, neuralgia, and scurvy. On the 13th of March, an old sailor, who was both scorbutic and anæmic, was attacked while climbing the mast; five days later three others were attacked in the same manner; and between the 18th and the 23rd two others. The corvette remained at St. Helena ten days, and during this time the health of the men improved and there were no fresh cases of angina pectoris; but as soon as the vessel got out to sea again—as soon almost as the voyage was resumed—six new cases broke out, in persons all of whom were very decidedly scorbutic. M. Gélinaeu, after detailing the individual cases, gives the following general description of the disorder. The pain begins invariably at the sternum, and is acute in proportion to its vicinity to the heart. It is behind and to the left of the sternum generally. In one instance it began at the basis of the thorax, in another over the musculus rectus, in a third in the right hypogastrium; in all cases it shot in unbearable pangs towards the heart, and usually these pangs were accompanied by a distinct dread of death. In the majority there was, in addition, pain in the left shoulder. The patients sought relief by drawing deep breaths, and by bending towards the left side. The pulse generally was retarded and small, and not unfrequently this condition of the pulse was accompanied by palpitation and violent action of the heart, as judged by the strong impulse of the apex of the organ against the side. During the attack the patient dared not venture to speak, and for some time afterwards his words were scarcely audible and the fewest possible. Eructation, with a feeling of relief, followed the attack in eight cases; vomiting, without much relief, in three or four instances. Once the attack was followed by retention of urine. No

attack happened during the night, and the most common time during the day was during the afternoon *siesta*. After awhile any effort, or the act of eating, was a sufficient exciting cause. The treatment consisted in the application of dry cupping glasses with various anæsthetic applications externally, and with narcotics internally.

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 71.—*On the Prevalence of Diseases of the Liver among Europeans in India.*

By Sir J. RANALD MARTIN, C.B., Physician to the Council of India.

(*Lancet*, December 20, 1862.)

Whether as original or secondary affections, diseases of the liver are, in fact, a very frequent and most important class of diseases in the East Indies; much more frequent and important, indeed, than can well be represented in the official returns, civil and military. No official returns afford, under special heads, any approach to the actual number or frequency of liver diseases; for when these last occur as complications with, or as consequents to, fever, dysentery, diarrhœa, sun-stroke, or cholera, the liver disease remains unnoticed, the cases being usually classed or numbered under the heading of the primary disease, whether fever, dysentery, diarrhœa, sun-stroke, or cholera. Officers from India present themselves every day in whom the only affection existing is the hepatic one; but the headings and the descriptions refer, often solely, to the primary diseases—namely, malarious fevers. It thus happens that the numerical hospital returns and private cases do not yield anything like an approximation to a true estimate of the amount of hepatic disease existing in our Eastern possessions. Speaking of the fevers of Hong Kong, Dr. Rutherford says that “nearly every European infantry soldier who has for any length of time been stationed in the island suffers from enlarged liver and spleen;” and thus have we throughout the East thousands of cases of fevers, remittent and intermittent, the terminations of which, in invaliding or death, are referable immediately to hepatic disease.

All Indian medical and statistical observers, as Johnson, Jeffreys, Morehead, the two Macphersons, Chevers, Waring, Ewart, and Moore, have commented on the influences of age, sex, constitution, habits of life, and course of service in producing sickness and mortality from hepatic diseases amongst soldiers in the scenes of their respective employments; and each and all have remarked on the greater liability of the private soldiers and their wives as compared to their officers, but especially on the comparative immunity of European children of both officers and men. Diseases of the liver are also comparatively rare in females belonging to the better class of society.

The fact is, that the European soldier's child in India is not over supplied with animal food ; it does not visit the canteens nor yet the bazaars ; there is no alcoholic stimulation—no repletion, and its instinctive impulse to movement prevents its lounging on cots in close, ill-ventilated barrack rooms ; and hence its exemption from diseases of the liver.

In Bengal, during the four years ending 1853-54, out of 84,143 men, there were 4626 admissions and 261 deaths ; out of 2970 officers there were 150 admissions and 3 deaths ; out of 7941 women there were 155 admissions and 9 deaths ; while out of 9255 children there were but 5 admissions and no deaths from diseases of the liver.

Mortality.—The deaths immediately from hepatitis, not including those by its complication with fever, dysentery, sun-stroke, and cholera, which have never been estimated in figures, are numerous ; and so, remotely, are those from ruined constitutions, after retirement from active service, and of which there can be no account. When we see, as instanced in the second part of Sir Ranald's work, *On the Influence of Tropical Climates*, officers, civil and military, dying of hepatic disease in London, after a residence at home of twelve to fifteen years, we are forced to conclude that, with the invalided soldier, his habits of life being considered, the influences which produce such diseases are even greater than with persons in circumstances, position, and habits of life so much above him.

In the acute congestive and inflammatory diseases of the liver, as they occur in the East Indies, the danger to life is so imminent that the case must, within a few days, terminate in recovery more or less complete, or in death more or less immediate ; while, in the chronic forms of hepatic diseases, the protracted distresses and injuries to health are very great.

Acute inflammation of the liver is essentially a disease of the sultry plains, and its severity may be judged of by the fact that the chance of death is greater from one such attack than from thirteen of fever of various forms as they ordinarily occur in India.

"In India," says Sir James M'Grigor, "when patients whose condition of life permits them to take a voyage to Europe are in this state of disease" (the chronic form of hepatic disease), "they never fail to take it, and most commonly are recovered by it ; but there is no hope for the poor soldier or sailor." Dr. James Johnson adds, that "they waste away and die for want of the only remedy that possibly could arrest the hand of death—change of climate."

Out of an aggregate British force of 211,998 men, serving in Bengal from 1812 to 1832, there were 14,015 admissions into hospital on account of diseases of the liver, of whom 924 died immediately. From 1833 to 1854, out of an aggregate force of 331,775 men serving in the above Presidency, there were 18,765 admissions, and 1345 deaths from diseases of the liver.

ART. 72.—*A Remark on the "Bilious Tongue."*

By Dr. ANTOINE CROS.

(Gaz. Hebd. de Méd. et Chir., January 2, 1863.)

In an excellent paper on the value of the diagnostic signs furnished by the inspection of the tongue, Dr. Cros says that it often happens that those coatings of the tongue which are looked upon as bilious, because they are of a yellowish or greenish colour, are not only not bilious, but not even yellowish or greenish. After having scraped off some of the matter forming these coatings, he found that it presented a greyish tinge, more or less marked and uniform, and that it preserved the same characters when examined *in situ* upon the tongue through a small opening in a card, which card has been placed upon the tongue. The yellowish or greenish colour which seems to point to a bilious origin, M. Cros looks upon as due to the fact that the tongue is of varying shades of red at the point, the edges, and under the coatings, and that these latter, which occupy the centre of the organ, give out colours which are of shades complementary to red.

ART. 73.—*A Remedy for Sea-Sickness.*

By Dr. MORLAND HOCKEN.

(Gazzetta dell' Associazione Medica, No. 1, 1863; and Gaz. Hebdom. de Méd. et Chir., April 10, 1863.)

Dr. Hocken, a surgeon in the royal navy of England, has spent a good deal of his time occupied in two voyages around the world, in testing the value of various remedies for sea-sickness. With this view, he divided his men into groups of ten, and treated each of these groups differently. Chloroform, creosote, effervescent drinks, hydrocyanic acid, alkalies and their carbonates, morphia, alcoholic drinks, &c., had each their turn in these experiments. The result is that creosote and hydrocyanic acid are very efficacious, but not so efficacious as the following mixture:—

R Acidi hydrochlor. dil. fʒij. 8 grammes.

Acidi nitr. dil. fʒj.

Acidi hydrocyanici (Scheele) ℥ xvj.

Magnesiæ sulphatis, ʒiv.

Aque ad, ʒviij.

Capiat cochl. ij, ampla tertia v., quarta quaque horâ.

This mixture answered especially well in a case where uncontrollable sickness resulted from the combined action of pregnancy and sea-sickness, and where abortion had been seriously thought about before recourse was had to the mixture.

ART. 74.—*A Case of Biliary Fistula opening externally.*

By Dr. COCKLE, Physician to the Royal Free Hospital, &c.

(Medical Times and Gazette, May 10, 1863.)

So late as the year 1851, the best special historian of this lesion, Fauconneau-Dufresne, at the time he published his monograph upon the calculous disorders of the liver, could only collate twenty-six cases of biliary fistula, from all causes, during the entire historic period. Even in these some were wanting in scientific precision. Of these twenty-six cases nineteen were caused by biliary calculi. Of these nineteen cases, in fourteen only was the sex indicated. It is remarkable that thirteen occurred in females. This numerical preponderance, however, harmonizes with the clinical experience that females are much more liable than males to the formation of biliary calculi, determined probably by their more sedentary habits and peculiar functions. With respect to this latter point, Fauconneau-Dufresne and the late Professor Chomel have seen cases of hepatic colic from calculi exactly coinciding with the menstrual period.

A brief analysis of these nineteen cases gives six recorded deaths, six permanent cures, two cases of permanent fistula. The result of the five remaining cases is not stated, but there are no grounds for inferring a fatal termination.

As regards the extent of the course of the sinus, it varied within wide limits. In some cases it was almost direct; in others long and devious, opening in the umbilical, or even inguinal region. In one case of Petit's, a director used after death (the patient refusing such examination during life) passed five inches obliquely upwards towards the gall bladder, where it met a firm resistance. The abdomen being opened, the gall bladder was found attached to the abdominal wall by a sort of hollow cord communicating, at one end, with the gall bladder, and by the other, with a small purulent *cul de sac* placed between the oblique muscles, and emptying itself through the external fistula. The gall bladder was densely adherent, hypertrophous, and contained calculi, but it is not named whether or not it involved the cystic duct. Of course, if the duct were closed, there could be no biliary fistula, in the true sense of the term. In two cases the fistula was bifurcate, each sinus containing a calculus. When a single stone only existed, and was removed, the sinus soon permanently closed. In those cases where others still remained in the bladder, the sinus, after closing, reopened at indefinite periods. In no case did the sinus permanently close so long as the bladder held any concretion. In nearly every case the liver was more or less enlarged, and in several bound to the abdominal wall by adhesion. In one case it was perforated by the fistulous sinus, and formed part of the track from the gall bladder to the orifice. In all the fatal cases the gall bladder was small, adherent, and greatly atrophied and degenerated.

Of the calculi, the smallest resembled a millet seed, the largest a goose-egg in size. The number, of course, being inversely as the size. The exact chemical constitution of the calculi is not, nor could

be, at that period stated. But their combustible nature being constantly alluded to, we may infer they were mostly of cholesterine.

Independently of these cases of biliary fistula collected by Fauconneau-Dufresne, three others, singularly enough omitted by him, are recorded by Walter in the Catalogue of the Museum of Berlin, published in 1796; and quite recently the list has been swelled by Oppolzer, the celebrated professor at Vienna, whose paper is published in the *Medico-Chirurgical Transactions* of Vienna for 1860. Dr. Cockle regrets that he has not time to embody the results in this communication.

CASE.—Hannah E., aged 59, widow, of sedentary habits, was admitted under my care at the Royal Free Hospital, towards the end of last year, with the following history:—She came of healthy family, and always enjoyed good health, with the exception of occasional pain and tenderness over the right side.

About nine days before her admission the pain in the side returned with unusual severity. It began soon after dinner, at the pit of the stomach, and extended over the entire body and side of the neck. She felt very chilly, and retched violently.

She is quite sure she was not jaundiced, and the state of her bowels was natural, but she remarked that her urine was dark in colour. After a day or two the violent pain and sickness abated, but there remained great tenderness and a swelling at her right side. Over this the skin became red and inflamed, and in less than a week after it burst, and a considerable quantity of green matter escaped with about fourteen calculi, such as I exhibit, apparently composed of cholesterine with striae of pigment matter, and of angular shape.

In the subsequent poultices three more calculi passed. The redness of skin and tenderness now rapidly abated, leaving an opening the size of a crow's-quill, situate about two inches in a slightly oblique line below the umbilicus. From the orifice was a constant oozing of nearly colourless mucus, which was somewhat increased upon pressure from above. A probe introduced readily passed obliquely upwards for nearly two inches and a half, when it came in contact with a solid body. Palpation, corresponding to this point, easily detected a firm tumour about the size of a small hen's-egg. Percussion, however, did not determine the peculiar chink occasionally met with where calculi exist in the gall bladder. The liver was very considerably enlarged. After some time she complained of pain and tenderness over the old spot, and a calculus was felt by the probe about one inch and a quarter from the orifice, and the skin again began to redden. After several ineffectual attempts on successive days to extract it with the dilating forceps Mr. Hill had constructed expressly, I was compelled to slit up the sinus as far as the stone, and readily removed it. It was the one now produced, the largest of the series. The wound quickly healed, and the sinus seemed for two or three weeks almost entirely closed. At the end of this time the old symptoms returned, and another calculus was detected about two inches distant from the outer orifice. Mr. Hill again tried to dilate the sinus and grasp the stone, but after a few attempts so much tenderness and purulent discharge followed that it was not considered safe to interfere further at present. Recently these signs have ceased, and the fistula has again relapsed into its former indolent state, contracting, and only discharging nearly colourless mucus. The patient has now returned to her ordinary occupation; but there can be no doubt that the sinus will again open from time to time, as many calculi are still in the gall bladder. Considering the difficulty of determining the amount of adhesion existing in such

cases, we must be extremely careful to use only the gentlest means in endeavouring to extract a calculus when high in the fistulous canal, whatever the amount of local irritation present. In a similar case, under the care of M. Robert, at the Hôtel Dieu, the attempt at extraction induced, from the disturbance of the adhesions, fatal peritonitis. In the case of Hannah E., the fluid obtained by pressure from the parts exhibited, under the microscope, numerous mucus or pus corpuscles, but no bile-cell or cylinder epithelium; and, on using the ordinary test of nitric acid, no evidence of bile existed, or, at least, of biliverdine: corroborative evidence of the entire closure of the cystic duct.

There is one residual point connected with biliary calculi deserving further inquiry, namely, the amount of sympathetic irritation of the kidney they may induce. Dr. Prout has already shown that severe renal disturbance is occasionally determined by these concretions. The following instance appears to support his opinion:—A few weeks ago, a middle-aged married woman was admitted under my care with symptoms probably indicating the passage of a calculus. After these had existed in great intensity for a day or two, she became deeply jaundiced. The liver and gall-bladder were greatly swollen and tender, leading to the inference that the seat of obstruction was the common duct. No bile entered the bowels. Just as the hepatic symptoms and signs were waning, she complained of pain in the back and bladder. Considerable dysuria existed, and blood in considerable quantity soon passed. So marked were these symptoms that Mr. Hill, learning that her father had undergone the operation of lithotomy, was induced to examine the bladder with the sound. No calculus, however, could be found. In a few days the urinary symptoms completely ceased, and she left the hospital well with the exception of slight jaundice. Now, it would be premature, considering the occasional association of biliary and urinary calculus, to assert that no concretion existed in the kidney, yet I am disposed to regard the symptoms and hæmorrhage as the result of intense sympathetic hyperæmia.

(E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 75.—*On the Natural Constants of the Healthy Urine of Man.*

By the Rev. SAMUEL HAUGHTON.

(*Dublin Quarterly Journal of Medical Science*, August and Nov., 1862.)

The general average of all the results obtained in the course of an elaborate and extended investigation is represented in the following table—a table which gives the natural daily constants of the average man, including both beef-eaters and vegetarians:—

Excretion.	Per Day.	Per day per Pound.
Urine	52·62 oz.	2·84 drachms.
1. Urea	493·19 grs.	3·331 grs.
2. Uric Acid	3·15 grs.	0·021 grs.
3. Phosphoric Acid	32·36 grs.	0·218 grs.
4. Sulphuric Acid	31·55 grs.	0·214 grs.
5. Chlorine	106·56 grs.	0·673 grs.
6. Extractives	175·27 grs.	1·183 grs.
7. Balance (viz., inorganic bases).....	115·73 grs.	0·827 grs.
Total Solids	957·81 grs.	6·467 grs. per lb.

ART. 76.—*On the Production of Carbonate of Ammonia in the Blood in Uræmic Poisoning.*

By DR. ALEX. PETROFF, of Dorpat.

(*London Medical Review*, November, 1862.)

Dr. Petroff has published some very important researches, which go to support the theory of Frerichs. The object of these investigations was chiefly to examine the validity of Oppler's statements founded on experiments of his own, in opposition to Frerichs. Oppler stated that Frerichs' second and third inductions were incorrect, and that the theory based on them was therefore groundless. He declared that after artificially producing uræmia in animals (by removal of the kidneys or by ligature of the ureters), he was unable to detect carbonate of ammonia in the blood or secretions; and, moreover, that the injection of carbonate of ammonia into the venous system of animals fails to produce any symptoms truly comparable to those of uræmia. He declared that in uræmia, as Hoppe had long since stated, the quantity of urea and extractives in the blood, and the quantity of extractive in the muscles, was abnormally increased. Dr. Petroff's present investigations were made with the sanction and assistance of Professors Bidder and Schmidt. Dogs and cats were chloroformed, and the operation of nephrotomy carefully performed. After recovery from the chloroform, the jugular vein was opened at various distances of time from the operation, and the blood received into two vessels, each containing absolute alcohol. One of these portions of blood was to be tested for urea, a few drops of acetic acid were added to this, and the mass allowed to stand for twenty-four hours in a cold place, filtered, and the filtrate evaporated to a dry mass, which was repeatedly washed with water and with absolute alcohol, and then set aside to crystallize over sulphuric acid. Prismatic needles of urea were thus obtained, and the presence of urea was confirmed by other tests. The other portion of blood, examined for carbonate of ammonia, was received also into absolute alcohol and then at once distilled, the distillate being received into dilute hydrochloric or sulphuric acid of known strength. The quantity of *free ammonia* was thus decided; while the ammonia in combination was estimated by the examination of the residue left from the distillation. The bile, the contents of the stomach, and other fluids of the body, were also examined for ammonia. Dr. Petroff also made a series of very careful experiments on the comparative effects of injections of ammonia into the blood, and the artificial production of uræmia. The following are the general conclusions at which he arrives:—1. When the kidney function is interrupted, carbonate of ammonia is formed in the blood. 2. Injection of carbonate of ammonia into the blood produces symptoms strictly comparable to those of uræmia. 3. The degree in which these symptoms appear, and their character, depend on the proportion of ammonia in the blood, and the circumstances in which it exists there.

ART. 77.—*On the Iodine Test for Saccharine Urine.*

By MM. TROUSSEAU and DUMONT-PALLIER.

(Journal of Practical Medicine and Surgery, June 3, 1863.)

A short time ago, while watching the effect of tincture of iodine upon urine containing biliary matter, Professor Trousseau and his chef-de-clinique, M. Dumontpallier, ascertained that the iodine was decolorized by diabetic urine. They say, "The diabetic urine, which was nearly colourless, on the addition of the tincture at first took the colour of barley-sugar; but to our great astonishment, the colour gradually disappeared, and in the course of a few seconds the urine again became perfectly colourless." They say, also, that they have repeated the experiment with the same urine, and also with the urine of diabetic patients, and always with the same result.

MM. Trousseau and Dumontpallier pour at once four drops of tincture of iodine into six cubic centimetres of the urine to be examined, experiment having shown that the healthy secretion is insufficiently tinged by the addition of two or three drops of the tincture. It is seldom necessary to increase the amount to five, six, or seven drops, to obtain a marked roseate hue. The chemical circumstances which promote the phenomenon have not hitherto been accurately determined.

In a clinical point of view, the authors conceive that it is even now fully demonstrated, that urine slightly acid, passed in the morning by a healthy or diseased subject, will be coloured in a marked manner for a variable space of time by the addition of four, six, or eight drops of tincture of iodine; whereas urine containing glucose, *very promptly* removes all trace of colour, as we have stated above, from four, eight, and even thirty-six drops of tincture.

On further inquiry, however, it appears that iodine is decolorized by healthy urine, though not quite so rapidly as by diabetic urine. Thus, M. Corvisart maintains that the decolorizing of the iodine tincture depends chiefly upon the action of the uric acid and the urates in the urine; and, consequently, that the decolorizing power of the urine, whether diabetic or not, is measured by the relative quantity of these salts in the urine; and M. Farge of Angers asserts that highly diabetic urine has a minimum power of decolorization; whilst a maximum power resides in febrile—uric acid—urine. M. Coulier examined three specimens of urine—one diabetic, one highly charged with uric acid, and another perfectly healthy; and he found that the diabetic urine (containing 28 grammes of sugar to the litre) did not decolorize more of the iodine than the healthy urine. With M. Corvisart, he considers the decolorization depends upon the action of uric acid. In a word, other evidence is wanting before iodine can be allowed to take a place among the tests for saccharine urine.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 78.—*On Eczema (including its Impetiginous, Lichenous, and Pruriginous Varieties).*

By Dr. T. M'CALL ANDERSON, Physician to the Dispensary for Skin Diseases at Glasgow.

(*Medical Times and Gazette*, May 9, 1863.)

Dr. Anderson follows in the track of Hebra and several other Continental dermatologists, and his views differ in many particulars from those which are generally adopted in this country. He says (we quote from a lecture delivered at the Dispensary to which he is attached as physician):—

"It has always appeared to me that much confusion exists in the descriptions given of eczema in most standard works upon dermatology. The restricted meaning which is commonly given to the word eczema has arisen, no doubt, from the adoption of the classification of Willan and Bateman, in accordance with which the elementary lesion of eczema is, of necessity, a vesicle. Defective as any classification of skin diseases must be, there can be little doubt that the anatomical classification is the most objectionable of all; for in this way are many dissimilar diseases brought together under one group, and violence is done to the symptomatology of many of them, owing to the necessity of placing them under the head of one of the elementary lesions. Thus, scabies is ranked with ecthyma and small-pox—diseases which have no connexion with one another whatever; and the first of these (scabies), though it often shows itself in the pustular form, is still more commonly met with as a vesicular or papular eruption, or as a mixture of all three.

"There can be no doubt, in my mind, that the best classification of skin diseases is one founded, not upon the elementary lesion, but upon the nature of the affection. This is the basis of the classification of Hardy, and of that which is adopted at this Dispensary.

"Now, to return to the subject of eczema, I feel pretty confident that those of you who study this disease carefully at the bedside without bias, will be forced to the following conclusions:—

"1. That the elementary lesion of eczema is not of necessity a vesicle..

"2. That it may be an erythematous state of the skin, a vesicle, a pustule, a papule, or a fissure.

"3. That impetigo, lichen, and prurigo are merely varieties of eczema, in which the elementary lesions are respectively pustules and papules.

"4. That cases of eczema are often met with in which an erythematous state of the skin, vesicles, pustules, papules, and fissures are met with in a combined form."

ART. 79.—*On the Diagnosis of Eczema.*

By Professor HEBRA.

(Wiener Medizin. Wochenschr., December 27, 1862; and British Medical Journal, March 7, 1863.)

Professor Hebra attaches a different meaning to the term *eczema* from that which has been generally accepted. He does not restrict the term to a disease characterized by the formation of vesicles followed by desquamation of the epidermis; but includes in it all disorders of the skin which are attended, in either their earlier or their later stages, by the presence of vesicles and serous discharge.

Eczema may be produced by artificial irritation of the skin. In this case, however, the results are not only vesicles and serum, but sometimes also mere redness and desquamation, sometimes small papular elevations of the size of pins' heads, sometimes even pustules and scabs. Again, there are a considerable number of cases in which the same person has on one part of his body desquamation from a reddened surface, on another red elevations as large as millet-seeds, and on another epidermic elevations filled with watery fluid; while other parts of the skin are partially denuded of their epidermis, and moistened with exudation. There may also be here and there yellow purulent points, of which some afterwards form brown or green scabs. Thirdly, the observation of the course of individual cases of eczema shows that many begin with the development of large and small vesicles, of which some become pustules, others burst, leaving exuding spots, while others are covered with yellow scabs, around which are formed papules and red desquamative spots. Towards the close of the disease, the whole of the pustules have formed scabs, which partly or entirely fall off after the drying of the exuded fluid, leaving red spots, more or less infiltrated, and covered with fine scales.

These facts Professor Hebra believes sufficient, in conjunction with clinical observation, to support the conclusion that the term *eczema* includes five distinct forms of disease. In support of this idea, he brings forward the following considerations.

1. The identity of the different forms of *eczematous* eruption may be proved by a simple experiment, which any one may make on himself or on persons at his disposal. Let an agent capable of producing artificial *eczema* (such as croton oil) be rubbed into corresponding parts of the skin of two persons, or into different parts of the skin of the same individual; the same quantity—say five drops—being applied to each part. In a few hours the effects will be manifested, but differently in different parts. On the scrotum and penis there will be oedematous swelling and redness, accompanied in many cases by innumerable vesicles; on the face the swelling will be more marked and the vesiculation less; while on the skin of the extremities the hair-follicles are swollen and raised above the level of the skin, and red elevations appear also here and there. If the application be not repeated, the swelling, vesicles, and red elevations disappear in a few days, leaving only slight redness and desquamation as indications of the inflammatory process that has been set up.

But if the croton oil be again applied to the same parts on two, three, or more days in succession, the disease produced is increased in intensity as well as in extent. The eruption on the parts themselves is increased, the red elevations being increased in number, and forming vesicles in consequence of the greater amount of sub-epidermic exudation; and the parts surrounding those in which the application has been made also become covered with an efflorescence similar to that which followed the application made on the first day. When artificial eczema has reached this degree of intensity, it does not end in simple desquamation; but in most cases the vesicles burst, and leave exuding spots; while the contents of other vesicles become purulent. As a consequence of the formation of the pustules, the skin surrounding them becomes more red, swollen, and painful. Having reached this point, the eruption recedes. The vesicles and pustules gradually dry and form scabs, which are thrown off by the new epidermis, leaving red patches, more or less infiltrated, and covered with desquamation.

If these results of the application of croton oil be analysed, they will be found reducible to five principal forms—viz., red elevations and vesicles, produced by the first application; red exuding patches, produced by the continued operation of the same irritation; pustules and crusts, arising from the metamorphosis of the elevations and vesicles; and finally, the red desquamating patches left after the removal of the other forms of efflorescence. These conditions are represented in several species of eczema to which authors have given names, and which Professor Hebra thus arranges in the order of their intensity: 1. *Eczema squamosum* or *Pityriasis rubra*; 2. *Eczema papulosum*, also called *Eczema lichenoides* and *Lichen eczematodes*; 3. *Eczema vesiculosum*—the *Eczema solare* of Willan; 4. *Eczema rubrum seu madidans*; 5. *Eczema impetiginosum*—*Eczema crustosum* of some authors.

Whether the same irritant will produce the same form of eczema depends on the quantity and strength of the application, as well as on its duration. Differences arise also from the part to which the irritant is applied—the skin of the genital organs, the face, and the flexures of the joints being most susceptible; and from the state of health of the individual, it being often impossible to produce an eruption by irritating the skin of a person in health, while, if he fall into a diseased state, an eruption is readily called forth.

2. When eczema is spread over the scalp, face, trunk, and extremities, either continuously or in patches, it rarely happens that the same form of eczema is developed on each part. For example, the scalp and the skin of the face may be the seat of impetiginous eczema, while the integument of the muscles of the ears, the nape of the neck, the axilla, and the flexures of the joints are covered with eczema rubrum, the limbs with eczema papulosum and vesiculosum, and the trunk with eczema squamosum. It is more reasonable, Professor Hebra holds, to regard these as forms of one and the same disease, than to diagnose the eruption on the scalp as *porrigo* or *tinea mucosa* or *granulata*, and that on the face as *porrigo larvalis*, *impetigo faciei rubra*, *crusta lactea*, *crusta serpiginosa*, or *melitagra flavescens* and *nigricans*; while the word eczema is confined to the vesicular exuda-

tive eruption on the trunk and extremities. The terms *impetigo* and *pityriasis rubra* he regards also as having been adopted without reference to the eczematous appearances preceding the conditions which have received these designations.

3. Clinical observation affords the most satisfactory confirmation of the view here adduced. In one case, eczema begins with the eruption of vesicles; in another, the formation of vesicles is preceded by the appearance of red desquamative spots or papules. Or both papules and vesicles may be simultaneously developed; and of the latter, most rapidly form pustules, the contents of which as rapidly dry into yellow crusts. Hence it is also evident that vesicles cannot be regarded as the sole form in which eczema at first appears. The appearance of the eczema also undergoes changes in the course of the disease. After the falling off of the crusts, the appearance known as *eczema rubrum* may be left; while, where vesicles have been at first developed, the outer layers of the epidermis are thrown off, leaving the *rete mucosum* denuded and throwing out exudation. Again, when the exudation ceases, the parts pass into the infiltrated, red, desquamating phase, known as *eczema squamosum*, or *pityriasis rubra*. Professor Hebra observes, in conclusion, that this view of the metamorphosis of eczema is more probable than the idea which some have held of the conversion of one disease into another—of eczema into *impetigo*, *porrigo*, *tinea*, *pityriasis rubra*, *melitagra flavescens*, and the like.

ART. 80.—*Case of Pellagra.*

By Dr. WILKS, Assistant-Physician to Guy's Hospital.

(*British Medical Journal*, January 31, 1863.)

A case of pellagra occurring in this country does not appear to be on record, except this be one; and only four cases are to be met with in French medical literature as occurring in France. Dr. Wilks is not very positive in his diagnosis, but he says he can make nothing of the case unless it be one of pellagra.

The disease, as it is witnessed in the north of Italy, is an affection of a very chronic nature, continuing for several years, characterized by a general wasting of the body, and by a rash which covers the exposed parts of the skin, commencing as an erythema, and progressing into a lichenous or eczematous eruption. At a later period, sores sometimes appear, impairment of digestion, and nervous symptoms, as paralysis of the limbs. The cause of the disease appears at present not decided, it having been thought by some that it is due to exposure to the sun, from the fact of exacerbations occurring in the summer time; by others, that it is due merely to poverty, as it does not prevail amongst the rich; but these and similar supposed theories are refuted by the remembrance that such causes are not peculiar to Lombardy, but exist where pellagra is quite unknown. The most recent theory places the cause in the consumption of diseased maize, the corn being affected somewhat after the manner of the ergotised rye.

In four cases which occurred in La Charité at Paris, some years ago, which are described by M. Willemin, and which appeared to be true examples of the disease, the skin was covered either with an erythematous, a lichenous, or eczematous rash. This was on the face and arms, and had a thickly defined margin; in two cases a diarrhœa existed; in one the gums were spongy, as in scurvy, and in some there was a weakness of the extremities. The pulse was feeble in all, and in one case, after the patient had been lying down, was reduced to thirty beats in the minute.

The peculiarities of the disease appear to be: in the first place, this cutaneous eruption, which is remarkable in affecting the parts exposed to the light, as the face, neck, chest, backs of hands, arms, or feet, the redness ceasing abruptly at a line where the covering of the clothes commences; the skin also becoming hard and dry like parchment, and in some cases bullæ appearing. Next in importance to the cutaneous rash are various nervous symptoms, as numbness or pains in the limbs, and sometimes a veritable paralysis, as in myelitis of the spinal cord. It is said by some writers that in very bad examples of the disease a melancholy overtakes the patient, determining to suicide, especially by drowning, and to this has been given the special name of *hydromania*. Other writers, however, are ignorant of the existence of this propensity. As regards the pathology of the disease, nothing is known; some have regarded it as a chronic *gastro-enteritis*; whilst others, at a loss to discover the seat of the disease, have been content to style it *morbus chronicus totius corporis*.

CASE.—The case is that of a woman, living at Glastonbury, and intimately known to Mr. Malton, of that place. This gentleman sent the case up to his relative, Dr. Charlton, of Dartford, who again brought her to me for an opinion as to the nature of her complaint, the obscurity of which suggested to the former gentleman, I apprehend, that there might be some disease of the suprarenal capsules. She was married, aged 33, and had three children. She informed me that she had been ill for six years, having become very thin, and being covered in parts with an eruption. She said that her illness had commenced five and a half years before, in the month of May, after exposure to the sun. These statements were not extracted from her by leading questions; for it was not until after she had left my house, and I was revolving the case in my mind, that the idea struck me, that it was one of pellagra. Soon after this exposure to the sun, a rash appeared on the face and arms, and subsequently she lost flesh and became weak. This rash, she said, had continued ever since with varied severity, being aggravated in summer, and better in winter. Her general appearance was very remarkable, presenting as she did a withered and emaciated look, reminding one of a child in the last stage of marasmus; all the outlines of the face were very prominent, the cheeks were sunken in and the temporal muscles much wasted. The face was covered with a brownish-red rash, not uniform in amount, but patchy; this was of a papular or lichenous character, with a slight furfuraceous covering. The neck was slightly affected in the same manner; the forearms as far as the elbows were covered with a similar rash; the skin being rough and affected by a brownish-red eruption. This appeared to correspond to the papules of the skin, and gave a hard dry feel to the touch. The backs of the hands were similarly affected, and, in fact, all those parts which were exposed, whilst those which were covered were

free. Her limbs were wasted, and the joints of the elbows somewhat contracted, not admitting of full extension. On the face there was also the scab of a pustule, and on the arm was another similar one. On the right forearm there was also an ulcer, the seat of a former pustule, and which was unhealthy-looking and bleeding. I could discover no disease in the body, for tuberculosis suggesting itself, I looked for phthisis and other manifestations of it, but nothing of the kind could be discovered. I also examined for enlargement of the cervical glands, which might have obstructed the thoracic duct; nothing could be felt in the abdomen, although I surmised the presence of mesenteric disease. The only nervous symptom was weakness of one arm. The gums presented no marked scorbutic appearance, but were rather too red and swollen. There was no diarrhoea, and she stated that her spirits were good.

It will be seen that the symptoms in this case very closely correspond to those described as belonging to pellagra; and, therefore, no surprise will be felt when I suggested to Dr. Charlton the nature of the disease. If it should not be of this kind, it will be found probably due to some morbid condition interfering with the due supply of chyle to the system; and in connexion with this, it will be remembered how harsh and withered the skin becomes in children who are wasting from mesenteric disease. It is not surprising, therefore, that the disease has been called (according to Dr. Peacock) *marasmus pellagrosus*, or *tabes pellagrosa*.

Within the last few days, Mr. Malton has written to say that the patient is going on much the same; the wound in the arm is still ulcerating; and she has lost almost complete power of the same extremity. The disease of the skin is better (winter time). Mr. Malton will, no doubt, report the further progress of the case.

ART. 81.—*Two Cases of Kelis, with Remarks.*

By Deputy Inspector-General LONGMORE.

(*Proceedings of the Royal Medico-Chirurgical Society*, February 24, 1863.)

Dr. Longmore begins by referring to a paper by the late Dr. Addison ("Abstract" xix. p. 103) on the Keloid of Alibert, and on a form of this disease which Dr. Addison designates True Keloid, on the supposition that it is of a distinct character. The two cases under consideration are striking illustrations of these two forms of keloid disease, but they do not, in Dr. Longmore's opinion, confirm the notion that these two forms are essentially distinct.

CASE I.—In this case the disease was developed after punishment by flogging, of a comparatively light nature. There was scarcely any laceration of the skin. About three months after the date of punishment the soldier noticed a growth upon the spot where the flogging had been chiefly received. This growth gradually increased from a small round tubercle to a large flat mass, nearly as large as a man's hand. It was not accompanied with pain, but there was irritability, itching, and tenderness when the part was subjected to the pressure of the cross-belt and weight of the knapsack. On the front of the patient's chest were several small tumours, evidently of the same nature, but the date of the first appearance of those growths could not be ascertained.

CASE II.—The disease in this case was more extensive than had been described by any writer hitherto, and presented a most remarkable appear-

ance. It was of the kind which Dr. Addison considered should be separated from the former variety under the name of "true keloid." The patient, a cavalry soldier, of strong, powerful frame, went to India with his regiment in November, 1857. Five months afterwards, at the commencement of the hot season, he began to suffer from lichen tropicus, in common with all the other men of the regiment. About a month afterwards the keloid disease began to show itself, in the form of a few prominent red tubercles on the right forearm. It next appeared over the middle of the sternum, and thence extended gradually towards the two sides of the body. At the same time it appeared on the left shoulder and various parts of the back, and continued to spread until it had covered the entire dorsal surface of the body. The skin of the face was also affected.

The physical characters and accompanying symptoms of the disease in both instances are fully described in the historical accounts of the cases. Their characteristic features are also represented by drawings and photographs.

The writer considers that no two more striking examples of the two species, "true keloid" and "spurious keloid," according to those who designate them as distinct species, could be met with. He argues, however, that in the case in which the kelis was excited by flogging, the comparative slowness of the punishment, the rarity of such a consequence, but more particularly the presence of the keloid spots in their favourite habitat—the skin covering the anterior part of the chest—established the constitutional nature of the disease, just as fully as it was established in the second case, where it appeared to follow the excitement of the prickly heat. The fact, too, that the general characters of the diseased growths in both instances were alike, and that in each instance the hypertrophy was greatest where the pressure of the cross-belt was chiefly exerted, confirms still further the identity of their nature. Hence the deduction that, both the patients being of a keloid diathesis, the difference in the mode of distribution, and in some of the features, of the diseased growths in the two cases could be accounted for by the different natures of the exciting causes.

No treatment appeared to exert permanent beneficial change in either case. The fact that the pressure of the cross-belt acted in each instance as a stimulus to increased growth was held to be a sufficient argument against treatment by continued pressure, which had been recommended by some surgeons. The evidence of the constitutional origin in the case where the chief tumour was isolated upon the left shoulder, as well as the nature of the immediately exciting cause, had counter-indicated any attempt at cure by extirpation.

ART. 82.—*On Verruca Necrogenica.*

By Dr. WILKS.

(*Lancet*, October 25, 1862.)

At one of the meetings of the Pathological Society, held during the past session, Dr. Wilks exhibited some wax models of the hands of a young man who has been employed in the post-mortem room,

in order to illustrate a peculiar affection of the skin produced by the acrid fluids of the dead body. The knuckles of both hands had upon them brown, circular, raised patches of morbid epithelium, giving the appearance somewhat of epithelial cancer. The chronic and obstinate nature of these warty epithelial excrescences is most remarkable; for if removed, or portions be picked off, they again grow, and remain for years. Dr. Wilks has on more than one occasion recognised this disease in strangers and suggested its cause, for which reason he believes it to be peculiar and characteristic; at least, he knows of no other irritants which produced exactly the same effect. There is no name already in use which is strictly applicable to this condition, as epithelioma, lepra, and such terms are already in use for definite affections; and the name *verruca necrogenica* is suggested as appropriate.

ART. 83.—*A Case in which Arsenic was discharged from the Skin long after its Internal Administration for a Cutaneous Eruption.*

By Dr. J. M. BARRY.

(*Dublin Medical Press*, May 6, 1863.)

This case was read before the Surgical Society of Ireland a short time ago. The discharge of the arsenic in this manner is a curious fact, but it is not the only point of interest. On the contrary, it may be asked whether the arsenic had not something to do with the paralytic symptoms; for such a question is suggested by more than one case which has come under our own notice.

CASE.—During a visit to London last summer, I was requested to give an opinion on a case which I have subsequently treated through the medium of correspondence, and some of the most interesting points connected with which I beg to have the pleasure of briefly stating for the consideration of this meeting.

The patient is a stout, florid young woman, about thirty years of age. Her complaint, an ulcer, situated on the inside of the right leg, midway between the knee and ankle, about the size of a crown-piece, but irregular in form, and nearly half an inch in depth. The discharge was considerable in amount and unhealthy in aspect. Her most urgent symptom, acute pain occurring in paroxysms, most frequent at night. For four or five inches round the sore the cuticle was thickened, discoloured, and rough. On this surface numerous small black specks were visible, which from time to time were discharged. They had made their appearance about a year since, and varied in number and size. It is the circumstance of arsenic being detected in these concretions which induced me to put the case on record.

The sore had existed for nine years, during which time numerous plans of treatment had been adopted ineffectually. Of late the patient had given up all hope of being cured, and merely sought alleviation of pain and discomfort by dressing with simple ointment and a linseed-meal poultice occasionally.

"On the 23rd of March, 1853," the patient states, "I went to Mr. H. for the first time, with the hope of having an eruption on my face cured. I had always been healthy, and at this time had nothing the matter with me

but the eruption. I was tolerably stout and strong. He gave me some medicine, like water in appearance, and directed me to take twenty drops in water two or three times a day, and to see him twice a week. I visited him for four months, taking the medicine in the meantime; but on attempting to get up one morning I could not stand, my knees bent under me, and I fell on the floor. I was placed in bed, and remained there three months, Mr. H. attending me. I had quite lost the use of my limbs from the hips. My hands were closed, and I had not the power to open them. My sight was much affected, and one eye was turned in my head. To my temples leeches were applied, blisters and mustard-poultices were put upon my chest, I was cupped on my back thrice, and salivated several times. Large boils broke out on different parts of my body. My limbs being perfectly useless, Mr. H. suggested crutches, and they were procured for me; but as I had not the power of lifting my legs, they were useless.

"At length a friend recommended me to obtain a plank about ten feet long, to be supported on chairs. I was to sit upon the centre of it, and keep my feet upon the ground. The springy motion of the plank moved the muscles of my legs, and I think from this I derived much benefit.

"In a little time I commenced walking about, but shortly after an ulcer broke out in my leg. I then went to Dr. P., at that time residing in Grosvenor-street; and to my great surprise he told me that I was suffering from the effects of arsenic, and that most likely it would be years working out of my system.

"I was under his care for several months, but as my leg did not heal, I discontinued going to him, and since have dressed the sore with some simple ointment, or poulticed it. These are the facts of the case as nearly as I can recollect, but I regret to say my memory is not nearly so good as it was previous to my taking the arsenic."

At the time I saw the lady last summer, her general health seemed to be tolerably good, her digestion was active, and appetite not impaired. It is true she seemed rather nervous; she complained of restlessness, and her pulse was accelerated; but still the irritable ulcer on her leg seemed to be the main and almost only indication of the fearful shock her constitution must have undergone from former illness, and the very active treatment she had been subjected to. For the sore, I ordered a weak nitric acid lotion containing opium. To the thickened and discoloured skin around, I applied tincture of iodine, and subsequently glycerine. For internal use, I prescribed iodine, iodide of potassium, and sarsaparilla. In a few days the patient seemed much better, the ulcer presenting a healthier aspect, the discharge less, and the pain relieved.

As I was about returning to Dublin, we arranged that she should correspond with me weekly. I was anxious she should call in some medical adviser who could personally watch the progress of the case. But this she declined. Three weeks having elapsed, she wrote to say that the treatment decided on had been continued regularly, the ulcer healing rapidly, and that she suffered but little pain from it; but unfortunately, as a drawback to this pleasing report, an ulcer had broken out on the left leg, nearly opposite to the sore on the right, and rapidly spread until it became about three inches in length and one and a half broad. While waiting for advice she had poulticed it. This I advised should be discontinued, and in lieu thereof I ordered a lotion consisting of opium, glycerine, and blackwash.

On the 26th of December, a week later, she wrote, saying the second sore was healing in the upper part, that the pain was much alleviated since she gave up poulticing and applied the lotion. On the 5th of January her report instructed me that sore No. 2 was still steadily improving, and many

of the black concretions were being discharged. At intervals she continued to paint the leg round the sore with tincture of iodine. At this period ulcer No. 1 was dressed with tincture of benzoin and lint.

January 12th : At this date I ordered nitric acid lotion for ulcer No. 2, to be applied warm. Miss G. found the change of dressing beneficial, and stated that she was decidedly better. On the 13th of January our satisfactory progress for a time ceased, the sore assumed a green appearance, with a considerable amount of unhealthy discharge, and became very painful, with flabby granulations arising in the centre. I directed her to touch these granulations with nitrate of silver, and apply warm-water dressing for a day or two and then resume the opium and blackwash lotion.

February 6th : We heard that the sore No. 2 was progressing very nicely, being dressed with the lotion last ordered applied warm, oiled silk, and a carefully-adjusted bandage. For a few days subsequent to this date things proceeded comfortably, when, alas ! my poor patient's condition altered much for the worse. She was attacked with erysipelas of the face. This untoward occurrence obliged us to modify our treatment for a time. We had to give up the iodine mixture, and No. 2 for the next week had merely warm-water dressing ; but time, rest, attention to diet, and one or two mild aperients, placed us once more on the high road to recovery. The erysipelatous attack passed away ; her spirits improved ; nervousness diminished, and she wrote to say that she felt much better in every way. Sore No. 1 only the size of a small pea. No. 2 healing from the edges and filling up. I recommended the application of collodion round the edge to produce contraction, strapping with soap-plaster, bandage and iodine mixture to be resumed.

24th : Sore No. 2 much smaller ; granulations bright red, looking very healthy indeed ; blackwash still agrees best ; iodide of potassium mixture discontinued in consequence of an unpleasant stitch in the left shoulder, throbbing pain in the lower jaw, and swelling of the tongue.

March 9th : Symptoms referable to the continued use of iodine have disappeared ; feels greatly better ; obliged to discontinue strapping, itching so unbearable. Sore No. 2 healed a quarter of an inch round the edge. Applied collodion again. Many of the black specks were being discharged from the right leg.

17th : Sore still decreasing in size. Ulcer No. 1 is now dressed with dry lint ; pays no attention to it, as it appears to be quite healed ; twelve black bodies discharged from the surface of the right leg yesterday. Ordered the iodine mixture to be resumed.

My friend Professor Davy having kindly offered to examine the concretions for me—a few of which I have the honour of submitting for the inspection of the Society—he informed me that they contained a considerable quantity of arsenic ; and it is this circumstance that induces me to bring the case under your notice. Professor Davy was so good as to promise me he would complete his analysis, and enlighten us as to the other constituents of this remarkable discharge from the surface of the body.

ART. 84.—*A Possible Cause of Baldness.*

By Dr. —.

(*Medical Times and Gazette*, April 18, 1863.)

A writer in the *Boston Medical Journal* suggests that the compression, by means of the hat, of the veins which return the blood

from the scalp is a common cause of baldness. He refers, in proof of this, to the much greater proportionate number of persons exhibiting baldness among the class who wear hats, compared with the lower orders who do so more rarely. Again, if we compress the frontal vein of a bald-headed person moderately with the finger, the scalp becomes speedily swollen and turgid; and, on inquiry, we shall find that he experiences an uncomfortable sense of fulness and constriction about the head whenever he wears a hat, especially in hot weather. It is true the hat does not induce baldness in all its wearers, those most liable to it being men of soft and pliable tissues, with large, superficial, and easily compressed veins. A large proportion of them have a long occipito-frontal compared with the bi-temporal diameter of the head—favouring the compression of the frontal and occipital veins.

ART. 85.—*A New Fungus in the Hair of Man.*

By Dr. ALOYS MARTIN.

(*Zeitschr. für rat. Med.*, Bd. xiv., 1862; and *Medico-Chir. Review*, April, 1863.)

Dr. Aloys Martin, of Munich, had under his care, in December, 1860, two infants, both girls, with typhus. The younger, who had only a slight attack, soon recovered, but had diarrhœa. The author goes on to relate: "The mother pointed out to me a spot on the back of the head of this child, in which the hair (become scanty and flax-like since her recovery from the fever) had become golden or yellow-red during the last six weeks. The patch was at the limit of the hair behind, irregular in shape, two inches and a half broad, by one and a quarter vertically. The hair looked as if it was smeared with a yellow-red pomatum; this substance appeared to cling in little lumps to the hair. In the patch, at one part of it, the size of a sixpence, the hairs were dropped out or broken off, the very short remains of hair that were left looked as if it had been singed, provided with black, brown, or dirty yellow-red ends, in the bald place, and remnants of hair similarly coloured were lying round it like powder. In the patch at other parts of it, the hair was thin, and broke off very easily at a touch close to the roots. The colour of the patch was shaded off." He supposed it was a fungus formation, progressing from a centre, and took specimens to Dr. Buhl. The following are the results of the examination:—

The single hairs, closely observed, were seen to be towards the root first yellow, yellow red, then blood red, brown red, and finally brown or even black, inasmuch as the hair increased in thickness. Even with the naked eye, it seemed that the change of colour did not belong to the hair itself, but to some foreign substance. So the microscope proved it to be. Magnified ninety diameters, the hair shaft appeared to be varicose, with swellings thickened from distance to distance through a mass round it, transparent and coloured by it yellowish or reddish. In certain spots one saw further how this had

its real seat under the upper cuticle of the hair, undermined it and lifted it off. Over the thicker spots the epidermis was of course quite loose after having been rent. In order to learn the real nature of the mass, the ordinary magnifying powers of 300—400 dimensions were not by any means sufficient. It appeared only punctate, or as if intermingled with molecular grains. By the immersion system of Hartnack, the investigator could alone study the condition accurately. The mass consisted of a structureless, jelly-like substance, in which cells were imbedded. The smallness of the cells is best shown by saying that, in spite of the high power used, the nuclei only consisted of a dark point. But that they were cells is clear from their analogy with the cells of the yeast plant and other similar formations.

Of its place in the vegetable kingdom the professor said the smallness of the cells proved it to have been a fungus. Because of its mentioned peculiarities, perhaps also because of its distinction from every other known epiphyte, in its special seat, the colour and minuteness of the cells, and the substance connecting them, he said it deserved to be made a species. He proposed the name *Zoogloea capillorum*.

It is mentioned, in conclusion, that germination by its transference to potatoes, apples, &c., could not be effected.

The treatment by simply washing with soap and water, after four weeks, had not destroyed or arrested the growth. The author purposely avoided corrosive sublimate or arsenic. After some months the soap-and-water treatment only had cured it. The new grown hair was even closer than before. No trace of the fungus could be discovered. The child, when ill, had been wrapped up by the mother in woollen cloths dipped in the water of a well, and the author thinks the fungus may have been so brought to her.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

CONCERNING INFLAMMATION.

ART. 86.—*On the Continued Application of Dry Cold as an Antiphlogistic to Injured and Inflamed Parts.*

By Dr. ESMARCH, of Kiel.

(*Jurors' Report of the International Exhibition for 1862.*)

THE means employed by Esmarch, of *Kiel*, for the continued application of *dry cold* as an antiphlogistic to injured and inflamed parts, are worthy of special notice. The essence of the application consists in the absence of moisture and the interposition of a bad conductor between the ice and the inflamed parts to prevent freezing. The theory is, *first*, that, as the constitutional irritation is known to have its origin in the wound, it may be best allayed by treating the wound; *second*, that as increased heat in an inflamed part is followed by a gradual increase in the temperature of the whole mass of the blood communicated to it during its passage through the vessels of the part, and as the continued abstraction of that heat must finally surpass its reproduction, a subsidence of fever may be expected in a corresponding ratio. Compound fractures, gun-shot wounds, and traumatic inflammation of the knee-joint complicated with extensive suppuration, appear to have been treated in this manner with scarcely any appreciable constitutional symptoms, while the use of warm cataplasms to the same class of wounds, in the same hospital, has led to disastrous results. The apparatus consists of a wide-mouth caoutchouc bag, closed by a large wooden roller, round which the open mouth of the bag may be tied, a ring being inserted in the centre of the roller, to receive a string for suspending the bag in cases where it is desirable to avoid the pressure of its weight.

ART. 87.—*Treatment of Ulcers of the Extremities by
"Sealing."*

By Mr. BARNARD HOLT, Senior Surgeon to the Westminster Hospital.

(*Medical Times and Gazette*, Dec. 6, 1862.)

In the treatment of ulcers of the legs a novel method has recently been adopted in the Westminster Hospital by Mr. Holt with the best effect. The treatment consists in excluding the air from the wound during the process of granulation, and this plan is found to assist materially in rapid cicatrization, when once a healthy action is set up.

The method of applying the dressing, as practised by Mr. Holt, is as follows:—The margin of the ulcer is covered with adhesive soap-plaster, half an inch wide, and a piece of oil-silk, large enough to cover both the ulcer and the plaster, having been carefully affixed by means of collodion, another edging of plaster is put on the margin. The transparency of the oil-silk allows the progress of the ulcer to be inspected with the greatest ease.

In some clinical remarks upon cases under treatment by this method, Mr. Holt observed, that "to render the treatment effective it was necessary to remove the sealing in accordance with the amount of discharge present. When the discharge is abundant, it may be necessary to remove the first sealing on the second day; but experience shows that, as the treatment is proceeded with, so the discharge gradually diminishes in quantity, and that the granulations which before the sealing were pale and flabby, become florid and vigorous. Hence, the second application of the sealing may usually be allowed to remain untouched for five or six days, and the third even longer, and so on until cicatrization is complete. No dressing of any kind is required beneath the oil-silk, which should be carefully secured, so as to exclude the air. By this simple method all irritating influences are avoided, the discharge is not too frequently removed, and the growth of healthy granulations is induced, leading to the rapid cicatrization of the ulcer."

We subjoin two cases illustrative of the treatment:—

CASE I.—William J., aged 22, a sailor, admitted, June 24, into Northumberland Ward, under the care of Mr. Holt, with an ulcer of the leg.

History.—About three years since, whilst on board ship, his right leg was crushed by some spars which fell upon it, and, for twelve months after this, pieces of bone came from the wound, which then healed up, and was quite well for six weeks. It then broke out into an ulcer again, and continued to enlarge up to the time of his admission.

On admission there was an inflamed ulcer on the outer side of the right ankle, about two inches long, and one inch wide, but not very deep. There was considerable inflammatory swelling of the surrounding parts, and he complained of burning, pricking pain in the ulcer itself. Ordered a linseed-meal poultice, and, a couple of days afterwards, a nitrate of silver lotion (gr. ij and ʒj).

July 1.—The ulcer was sealed.

4th.—Unsealed, and found to be diminished in circumference half-an-inch; re-sealed.

7th.—Dressings renewed. Discharges a good deal.

10th.—Ulcer much diminished in size.

14th.—Ulcer healing rapidly, and of very small size.

25th.—Discharged cured.

CASE II.—Martha B., aged 22, a servant, admitted, June 24, into Percy Ward, under the care of Mr. Holt, with ulcers of the legs.

History.—On December 28 last, she received a blow on the left leg, which gathered and was poulticed, and a large slough separated, when lotio nigra was applied. The ulcer continued to increase in size, and an ulcer appeared on the right leg, without apparent cause, about three weeks before her admission.

On admission there was an ulcer on each of the legs immediately below the patella. The largest, on the left leg, was of the size of the top of a small teacup, whilst that on the right leg was of the size of a two-shilling piece. They were both very deep, with ragged edges, and discharged freely. Ordered linseed-meal poultice.

June 26th.—Ulcers more healthy in appearance, and less painful. R Acidi nitrici dil., mxxv .; decocti cinchonæ, 3j ., ter die.

27th.—The depth of the ulcers much decreased, and their size diminished. The one on the left leg is about the size of a five-shilling piece, and that on the right about the size of a shilling. Both ulcers were "sealed" according to Mr. Holt's method.

30th.—Left ulcer of the size of half-a-crown, and the right of a sixpence. Ulcers re-sealed.

July 10th.—Ulcers unsealed, and found to be much smaller. The same dressing applied.

14th.—The left ulcer of the size of a shilling, and the right completely healed.

29th.—Discharged.

ART. 88.—*The Bite of a Rattlesnake treated successfully by large Doses of Whisky and Ammonia.*

By Mr. ———

(*The Medical Record of Australia*, December 24, 1862.)

Names, dates, and places are all omitted in the following account. We presume, however, that the editor of the journal in which we find the account had satisfactory evidence upon these points.

"A man," so runs the account, "had just been bitten. As he had no tobacco, I told him to fill his mouth with salt (grease or oil is better), and with all his might suck the wound. I then held a cloth steeped in hartshorn on the wound, to counteract the working of the poison. I next put thirty drops of hartshorn into a glassful of whisky, and poured the whole contents down his throat. Five minutes afterwards I repeated the dose, and again in other five minutes. I had now administered a whole quart of whisky, with ninety drops of hartshorn, and held it sufficient. The man was an Irishman, an old soldier, and took the thing very coolly. It was a great satisfaction to him when he heard that another man had killed

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the serpent. For three-quarters of an hour he sat quiet, and spoke about the bite with cold-blooded indifference, while I continued to renew the application of hartshorn to the wound in the finger. He said it was too bad that he should die of the bite of a poisonous snake, while I was astonished he could remain unaffected after such a dose of whisky. After about an hour he began to laugh, then to whistle, then to sing, and finally attempted to dance. It was now all right. I knew that the whisky had gained the upper hand of the poison, and for the first time intoxicated him. Five minutes after he was as drunk as Bacchus, beat on the ground with his feet, slept for half a day, and in the morning was well and at his work."

ART. 89.—*On the Employment of Position in Controlling Venous Hæmorrhage.*

By Dr. FRANCIS B. QUINLAN.

(*Medical Times and Gazette*, February 7, 1863.)

The plan advocated by Dr. Quinlan is one which was first employed by Dr. O'Ferrall, of St. Vincent's Hospital, Dublin, in 1845. It is intended to obviate the chances of that terrible hæmorrhage arising from the division of enormous tortuous veins while in a state of repletion—chances which are illustrated by the citation of two cases. In the first of these cases, a large scrotal tumour, weighing about fifty pounds, was removed by the late Mr. Liston, the veins being in an engorged condition. Upon the first incisions being made the blood flooded out, to use the words of the celebrated operator, "as from a shower-bath;" the patient rolled in exhaustion and agony from the table, and the operation was completed upon the floor; the patient collapsed, and was with difficulty restored by the energetic exhibition of stimulants. In Mr. Aston Key's operation, performed upon the Chinese Hoo-Loo, the results were similar, but, from the feeble Asiatic temperament of the patient, more disastrous. The operation lasted an hour and three-quarters, and the patient, who had shown some signs of syncope during its continuance, died immediately after its conclusion. It may be observed that in both these cases the genital organs were necessarily sacrificed in an effort to hurry the operation to a conclusion, in order to save the patient from impending death from hæmorrhage.

Dr. Quinlan then proceeds:—

"Results of this character, occurring in the hands of some of the first operators of the day, were sufficiently appalling; and it speedily became evident that, unless some means could be devised to diminish this excessive hæmorrhage, the removal of such tumours must, like the extirpation of bronchocele, be for the present abandoned. It was, therefore, with peculiar satisfaction that the profession learned, in the *Dublin Hospital Gazette*, of February, 1845, that a method of operation had been devised by Dr. O'Ferrall, by means of which he had removed an enormous scrotal tumour (fully equal to those

removed by Liston and Aston Key) without difficulty in eight minutes, and with the loss of only five ounces of blood; the genital organs being preserved, and the patient having made a good recovery, notwithstanding attacks of erysipelas and various other unfavourable circumstances. Such an announcement could not fail to be in the highest degree gratifying; and it became all the more so when it was found the importance of Dr. O'Ferrall's plan of operation was only equalled by its extreme simplicity. Observing the great change produced in turgid varicose veins of the leg by placing the patient upon his back and elevating the limb, and the immediate arrest of hæmorrhage from such veins which ensues upon the adoption of this position, it occurred to Dr. O'Ferrall that, if the enlarged scrotum were held up, a similar withdrawal of the vital fluid would take place, particularly as regards the enlarged and tortuous veins which were the principal sources of hæmorrhage. The result completely justified the accuracy of this expectation,—the more so as the hæmorrhage in these cases had been always observed to be principally of a venous character; the arterial hæmorrhage, in Aston Key's case, being estimated to be scarcely one-twentieth of the whole.

“ Since the publication of Dr. O'Ferrall's plan, a complete change has occurred in these operations, which have since been performed in rather considerable number, and with an ease and success more or less resembling that experienced in his case. I now recur to the plan, because in two instances of operation published during the present year (in one of which an Asiatic was the subject) it appears to me that the able and successful operators, although adopting the method, omitted, in their reports of the cases, to make due acknowledgment to the author; contrasting, in this respect, with Mr. South, who, in his splendid work on Surgery, gives due prominence to Dr. O'Ferrall's plan.

“ The application of this method is by no means limited to the removal of large scrotal tumours. On the contrary, it has been resorted to by Dr. O'Ferrall in cases of considerable innocent tumours of a vascular character; and in amputations he has obtained great advantages by loosely applying the tourniquet, elevating the limb, emptying it of venous blood by manipulation, and then tightening the tourniquet. The limb can thus be kept in a state of comparative anæmia while the amputation is being accomplished; and a loss of blood can be prevented, which, by deteriorating the general quality of the vital fluid, might have laid the foundation of subsequent disease. In fact, the value of a position by which the entrance of arterial blood into a limb will be retarded, and the exit of venous blood facilitated, is almost as useful in the performance of an operation as in the treatment of inflammation.”

ART. 90.—*On the Treatment of Nævus by Tartar Emetic.*

By Dr. ZEISSL.

(Wien Wochenblatt, No. 9, 1862; Medical Times and Gazette, Oct. 25, 1862.)

Dr. Zeissl has found that nævi of medium size are best treated by the application of tartar emetic, which is both safe and effectual. It is true that this substance has long since been used for this purpose, and without much success; but in the employment of cauteries it is not of so much consequence which caustics we use, as how we use them. Neither a solution of tartar emetic nor the ointment will produce the desired effect. A plaster should be made of from sixteen to eighteen grains of tartar emetic and one drachm of diachylon, and a considerable portion of this should be spread all over and somewhat beyond the nævus by means of the back of a strong knife, and kept *in situ* by strips of gummed paper. On the fifth or sixth day the entire surface of the nævus begins to suppurate, a crust gradually forming, which falls off in about fourteen days, leaving a most surprisingly slight cicatrix. If the suppuration is very profuse, we may replace the plaster by simple oil-dressing; but when it is not profuse, the plaster may be left on until it falls off. When the plaster becomes accidentally removed, wholly or in part, it must be renewed. Dr. Zeissl has repeatedly employed this application, both in children and in adults, without pain being produced. He has, however, never as yet resorted to it for nævi of the mucous membrane of the lips.

ART. 91.—*On the Importance of Tapping the Joints and Bursa Mucosa.*

By Professor INZANI, of Parma.

(Omodei's Annali, Nov. 1862; Medico-Chirurgical Review, April, 1863.)

The author begins by asserting the perfect harmlessness of puncturing a distended joint, even during the progress of acute inflammation. The fear of bad consequences following from the wound of the tendinous structures is a mere imagination of the ancients; nor does the air ever appear to make its entrance. The puncture may be made with a trocar or a lancet; the latter is preferable for superficial joints. The author has operated very frequently on the knee, several times on the elbow, occasionally on the carpus and ankle, and once only on the hip; no bad consequences ever followed. Pressure by means of a starched bandage should be made, and when the synovial sac refills, it should be again punctured before the distension has advanced too far. In this way a radical cure may be obtained. Examples are given in which large joints, principally the knee, were opened for effusions of blood, of serum in acute inflammation, of serum in chronic inflammation, and of pus—

usually with a successful result. But paracentesis should be avoided where the skin is much thinned and ulceration seems impending.

In the synovial bursæ, paracentesis has given equally good results. The examples which are given are those of effusion in the sheaths of tendons after accident (as the peronæi in sprains of the foot, the extensors of the thumb in falls on the hand), in which a puncture will give exit to synovial fluid mixed with blood, with much relief to the pain and abbreviation of the course of the disease.

The author believes that by these punctures chronic synovitis may often be arrested in cases which, treated by ordinary methods, would end in "white swelling," and that in dropsy of the joint the treatment by repeated puncture and pressure is as effectual and more safe than by injections.

ART. 92.—*On the Influence of Hope as a Stimulant in Military Surgery.*

By Dr. FRANK H. HAMILTON, Military Inspector U.S.A.

(*American Medical Times*, March 21, 1863.)

In an address to the graduating class of Bellevue Hospital Medical College, Dr. Hamilton says,—and his words deserve to be remembered:—

"Cold water and words of encouragement are excellent stimulants. Remember, gentlemen, that to the wounded and fainting soldier *hope* is a better cordial than brandy; and that you ought never to omit to offer words of encouragement, when it is in your power to do so."

ART. 93.—*On the Wire Compress as a Substitute for the Ligature.*

By Mr. JOHN DIX, of Hull.

(*Proceedings of the Royal Medico-Chirurgical Society*, Jan. 13, 1863.)

The wire compress—the subject of this paper—is a modification of the method of arresting hæmorrhage devised by Dr. Simpson of Edinburgh, and introduced by him about three years ago as a substitute for the ligature. The "acupressure," as it is called, has been tested by but few surgeons of note; and in London, especially, it is almost unknown and ignored. Although probably a real improvement on the ligature, it undoubtedly labours under certain inherent disadvantages, most all of which (it is believed) are obviated by the use of a fine wire of iron or of silver, instead of the steel needles of Dr. Simpson.

This idea was first promulgated in a paper on Acupressure published in the *Medical Times and Gazette* of June 2nd, 1860; and

first put to the proof in a case of amputation of the finger, September, 1860. In this operation two arteries were secured by wire, which was removed on the third day. The case did well: there was no bleeding, and very slight suppuration.

In the next case—Chopart's amputation, performed April 26th, 1861—five wires were applied on as many arteries: four of these were removed in forty-eight hours, and the other on the fourth day. It was found that the wire was easily applied, as easily withdrawn, and entirely effectual for the purpose it was intended to serve—namely, the arrest of the bleeding from the cut vessels. The patient, being the subject of constitutional syphilis, did badly. There was sloughing of the entire surface of the wound, and the flap was totally destroyed; notwithstanding which there was no hæmorrhage; but she died on the thirteenth day after the operation, of pyæmia.

In an amputation of the thigh, done September 21st, 1861, after Mr. Luke's method, there were seven bleeding arteries. Upon five of these the wire was used, and with the femoral artery the femoral vein was intentionally included; two very small branches were treated by torsion. This case did well. Seventy-two hours after the operation four of the "presse-artère" wires were withdrawn with perfect ease and without bleeding. The one on the femoral remained five days, when it, too, was removed without any difficulty and without a trace of blood. There was but little suppuration, and an excellent stump was the ultimate result.

These cases proved that this mode of securing arteries is practicable, efficient, safe, and manageable. It was also believed to possess a certain positive superiority over the ligature.

A ligature in a wound impedes union and induces suppuration. Cure, by primary adhesion, of a large wound—as, for instance, an amputation—is an event of extreme rarity, and this because of the ligatures. A thread of silk is, in fact, a miniature seton, and the whole number required in an operation make up one of considerable size, and can scarcely fail to lead to the formation of pus. Again, the ligature of necessity excites ulceration of the artery upon which it is tied; it cannot in any other way be got rid of. This is another unhealthy process, antagonistic of repair. In applying a ligature, the end of the artery is drawn out from its sheath, by which its natural connexions are disturbed and its vasa vasorum broken up; its coats also are lacerated and bruised. The ligature remains for an indefinite time, long after it is useful or necessary, and it is not unfrequently pulled at by the dresser before it has become detached. Its knot, often deeply buried between the flaps, cannot be withdrawn without tearing through adhesions, or damaging the granulations. All these are serious obstacles to the healing process, both in the stump and the artery itself, and much protract the period of cure. Moreover, the following is an interesting and noteworthy formula: Pyæmia is the offspring of purulent secretion, of which the ligature is an efficient and probable cause. Bleeding arises solely from ulceration of an artery, of which again the *primum mobile* is the ligature.

From one and all of these objections to the ligature, the "wire compress" is almost or altogether free. Thus, in accordance with a well-known pathological law, *it*, being a metallic substance, is freely tolerated by the living body, and has little or no tendency to excite suppuration or irritation. Neither does it cause ulceration of the artery. This is positively affirmed from actual observation of its effect, as witnessed in the sloughing stump before alluded to. It is applied without interference with the natural relations and vital connexions of the vessel. It is removed at any time, according to the will and judgment of the surgeon, without disturbance to the reparative action going on in the artery and in the rest of the wound, without futile premature attempts, and almost without pain to the patient. It is not liable to loose its hold, or to become detached too soon, as not unfrequently happens to a ligature applied upon a brittle or sloughing artery. Twigs of nerve accidentally included in the embrace of the wire are not injured and excited as by the tight strangulation of the ligature; and, if thought advisable, the veins are easily and safely occluded, along with the arteries.

Although this has been spoken of merely as a modification of acupressure, yet it is believed to be a decided and important improvement on "Simpson's skewers," as the needles have been irreverently called, and which are fairly open to the following objections. When several of them are required, the stump resents, as it were, being thus pierced through and through in various directions. From the injury thus inflicted, and from the obstruction to the capillary circulation caused by the pressure of the unyielding steel, arise much tension, cedematous swelling, and great pain; the pain especially has been found a very serious evil. Again, their projecting ends, and the puckering they cause in the substance of the flaps, interfere very much with that accurate adjustment of the cut surfaces and edges which so greatly aids the chances of union by adhesion.

The wire is free from all these shortcomings. It is thus applied. Take a piece of surgical wire six or eight inches long, and thread each end thereof upon a straight needle. Seize the bleeding mouth of the artery with forceps, and pass one of the aforesaid needles close on each side of the artery just mentioned, about a line above the points of the forceps, directly down through the substance of the flaps, so that they emerge at the cuticular surface, about half an inch distant from each other. Draw them both through together till the curve of the wire compresses the artery on the face of the flap. Now get rid of the needles by clipping through the wire close above their eyes, and also detach the artery forceps. Place a piece of cork, cut for the occasion, upon the skin, between the points of exit of the wire, and over this twist the wire tighter and tighter until the bleeding is arrested. Lastly, cut off the superfluous wire. All which is done quicker than described. Two or more arteries lying near together may be embraced by one wire; and, as has been said, the veins may be included or excluded at will.

The wire should be either of silver, or, what is much cheaper and

equally manageable, of the finest and softest passive iron. The generality of wire, as used for sutures, is too hard and stiff. The needles are about three inches in length, straight, and three-edged, with an eye adapted for carrying wire. Special care is necessary in threading the wire, that it is kept perfectly free from all twisting. The forceps are used, not to draw out the artery as when a ligature has to be applied (this, indeed, is to be particularly avoided), but merely as a guide to mark the exact position and course of the vessel. The cork is necessary to protect the skin from the pressure of the wire.

The withdrawal of the wire, which at first sight appears an insuperable difficulty, is perfectly simple and easy. It is thus effected. Clip the wire close to the edge of the piece of cork, and straighten out the curve it has formed, at its exit from the skin. Remove the cork, and apply instead the tip of one finger, with which press firmly upon the flap, making traction gently and gradually upon the other end of the wire. If this were roughly and hastily done, it might break up the adhesion which it is presumed has taken place between the surfaces of the flaps, and it is quite possible that a flexure in the wire might lacerate the artery in passing over it; but it is certain that none of these evils need happen with ordinary care and tact.

As to the period of withdrawal, further observations are desirable; but it has been clearly shown in numerous cases of acupressure, that for small vessels a few hours of compression are sufficient, and for the largest arteries a much less time than might be supposed. However, as a general rule, it is not desirable to disturb a wound in any way for from twenty-four to forty-eight hours, at the end of which time all wires commanding the secondary branches may be safely removed, and probably also from the large arteries; but, as a matter of prudence, it is well to keep a check upon such a one as the femoral for three or four days at least.

The descriptions hitherto given apply particularly to amputations; but the wire is equally applicable to many other operations.

In a postscript to this paper was related a case of excision of the testicle, in which it was used most satisfactorily upon the vessels of the cord. Two wires were required, and they were removed on the fourth day. Also a case of excision of the breast, in which three wires were applied, and removed in twenty-four hours. The wound, which was six inches long, healed kindly and rapidly—almost without suppuration.

Suppose the femoral artery needs to be secured for popliteal aneurism. The wire would be passed under the artery by means of a tubular aneurism-needle made for the purpose, brought out through the integument at a convenient situation, and then twisted upon a cork in the usual way. This same artery, cut in amputation, is securely closed in three or four days by the action of metallic pressure; consequently, in three or four days the wire might be removed. Meanwhile, it has not caused suppuration, or impeded the union of the wound, which ought, therefore, by this time to be completely healed; and more important still, the artery is not cut

through, as by the ligature, but its coats remain intact, and bleeding is impossible.

It will probably be found that the pedicle in ovariectomy may be conveniently dealt with by this method, the arteries being secured individually, and the entire stump also fixed to the abdominal wall by another wire. This idea has been already promulgated by Mr. Spencer Wells.

ART. 94.—*Horsehair as a Substitute for Wire.*

By Mr. THOMAS SMITH, Demonstrator of Anatomy at St. Bartholomew's Hospital, and Assistant-Surgeon to the Hospital for Sick Children.

(*Lancet*, Nov. 8, 1862.)

With a view of finding a material for sutures as unirritating and as unabsorbent as wire, but more easy of adjustment and withdrawal, Mr. Smith performed during last spring a series of experiments on animals to determine the suitability of horsehair as a substitute for wire in certain cases. The horsehair used was such as is ordinarily sold by fishing-tackle makers. The experiments were performed upon dogs. The general results showed that there was no appreciable difference shown by the tissues in their tolerance of silver wire and horsehair. Both materials were equally unirritant; yet there was a difference in favour of horsehair in the greater facility of its adjustment and subsequent removal.

ART. 95.—*On Syphilization.*

By Professor SIGMUND, of Vienna.

(*Gazette Médicale de Lyon*, Oct. 18, 1862; *British Medical Journal*, Oct. 18, 1862.)

Professor Sigmund of Vienna, the *Gazette Médicale de Lyon* says, has lately given the profession the benefit of his experience concerning syphilization. As professor, he has considered it his duty to study syphilization. In 1853 he tried it, but without satisfactory results. In 1858 he again experimented with it, and on this occasion in a more complete manner. In 1858 he operated during nine successive months on thirteen *syphilitic* subjects, twelve adults and one new-born infant. The twelve adults suffered from indurated chancres, papular eruptions, and syphilitic maculæ; the child had numerous tubercles.

M. Sigmund could only operate successfully with the pus of simple chancre. He never succeeded in producing chancres with the pus of indurated chancres. The inoculations were practised at first on the outside of the arms; then successively on the sides of the thorax, the thighs, and the legs.

Every three days, three, five, six, seven, or at most nine punctures, were made. No medicine was given, and the local treatment was simple attention to cleanliness.

M. Sigmund found that the inoculated chancres required three, four, and five months for cicatrization. Consecutive adenitis never resulted, and the cicatrization always took place in a regular way. In the leg, however, the ulcerations were longer in healing. The immunity was never perfect; it was only local; that is to say, chancres could still be inoculated on the leg, when the results of inoculation on the thorax were negative. But this partial immunity was only temporary.

M. Sigmund never succeeded by this practice in curing syphilis, although in one patient he had made as many as six hundred punctures. The negative results of his experiments may be classed as follows:—1. No cure; 2. Imperfect cure; 3. Partial recurrence of disease; 4. Complete recurrence.

These results, therefore, as far as they go, authorize us in preferring mercury, iodine, and other therapeutic agents, to syphilization, which leaves the patients covered with scars, and requires six months of inoculations.

M. Sigmund concludes with the statement that syphilization is not a dangerous practice, and that it has no evil effect on the constitution.

These results, M. Diday says, confirm experimentally the doctrines of the Lyonnese syphilitic school. They show, in addition to the duality of the chancre-virus, the existence of the mixed chancre; for it occasionally happened that M. Sigmund obtained soft chancres in inoculating syphilitic subjects with the pus of indurated chancres.

ART. 96.—*Report upon Syphilis in its Manifestations as a Constitutional Disease.*

By DR. JEFFERY A. MARSTON, Royal Artillery.

(*Proceedings of the Royal Medico-Chirurgical Society*, Feb. 21, 1863.)

After referring to a previous paper (*Abstract*, xxxvi. p. 188) Dr. Marston gives a short *résumé* of prevailing doctrines. Upon the subject of one attack of true syphilis affording an immunity against a second, some remarks are made illustrative of its general truth, to which, however, there were a few rare exceptions. Under this category a case is given in which a soldier had been treated twice during eighteen months for indurated chancre.

The writer next briefly adverts to the comparatively modern reaction relative to the contagious character of secondary syphilis. He then passes to the subject of secondary sores upon the penis, commencing either by a recrudescence in the cicatrix of a former sore, or upon some part not previously diseased. These he describes

under three heads:—1st. Such secondary lesions may commence as a circumscribed patch of purple or bluish redness, very slightly raised, from which the epithelium is shed, the surface becoming abraded, cracked, or covered with ill-formed and degenerating epithelial products. 2nd. This purplish-red spot, after becoming raised, takes on a chronic ulceration, similar to that of the scooped-out ulcers seen upon the tonsils. The first variety corresponds with, and often accompanies, a squamous or erythematous exanthem; the latter appears allied to the syphilitic tubercle, the ulceration of which plays so important a part in the evolution of syphilis in some subjects. The third variety appears as a persistence of the ulceration, or an unhealed condition of the chancre, which becomes transformed into a mucous tubercle, as described by Ricord. Of these he gave illustrative cases.

Upon syphilitic infection, contracted from some other source than contact with a virus obtained from a primary lesion, the following are cited as probable instances:—

1. A wife and two children. The former had syphilitic nodes and an ulcerating tubercle upon the lower extremity. Of the latter (who slept with her) one was an infant, in whom the disease was probably hereditary, though not congenital in its outward manifestations; the other was a girl of five years, with anæmia, non-ulcerated sore-throat, engorged post-cervical glands, cutaneous syphilids of trunk, with psoriasis palmaris. All had very restless nights. The husband lived separately, and was healthy.

2. The subject of disease was a military servant, a married man, who suffered from secondary and tertiary symptoms after intercourse with a woman known to be constitutionally syphilitic. Attempts to trace any primary lesion in this man failed.

3. The infection of a female by labial contact with diseased secretions from secondary lesions upon the lips of a male. The disease so produced in the female commenced as an irritable and indolent fissure upon the mucous membrane of the lower lip.

The writer next adverts to the evolution and succession of syphilitic symptoms as ordinarily witnessed among soldiers.

Under the cutaneous affections, he particularly remarks upon the mixed character of these; the majority of cases partaking of more than one affection at the same time, and upon the same or different parts of the body. Although great modifications in the future health and efficiency of the soldier were, doubtless, the result of syphilitic infection, yet cases of the more severe and intractable forms of constitutional disease were not very common.

With the view to prognosis, it is important to inquire, What relation, if any, has the severity of the primaries to those of the later symptoms? From a consideration of his own observations, Dr. Marston infers:—

1st. That the greater the induration, and the longer the period during which primaries remain unhealed, the more certain is it that the constitutional infection will be severe.

2nd. That the amount of ulceration, &c., of the primary sore stands in some relation to the worse and more intractable forms of secondary

lesions—*e. g.*, the pustular, erythematous, and rupitic eruptions,—the unhealthy ulcerations, nodes, and gummatous tumours.

The writer then passes to those cases in which there was an irregular evolution and succession of the stages and symptoms of syphilitic disease. Assuming that, when an indurated sore has been recognised, some specific treatment has been employed, and that soldiers are exposed to changes of climates, &c., we may infer, what is actually the case, that the constitutional symptoms would be irregular in their appearance and varied in their kind. As illustrative of his remarks, the author cites:—

1. A case of Hunterian chancre unhealed at the end of three months, when syphilitic rheumatism and a node upon the left parietal bone appeared.

2. A case in which, three years after a sore upon the external integument of the penis, a soldier suffered from two attacks of jaundice, anæmia, nodes upon the tibia, synovitis of left knee-joint, and rheumatism, without any history of secondary symptoms having been traced.

3. A case of repeated epileptic attacks, with violent pain in the head (upon which a node appeared), cured by specific treatment. The subject of the disease had suffered from repeated attacks of venereal disease, but not for any primary disease for two years and a half previously.

4. A case of chronic dyspepsia, slight icterus, pseudo-epilepsy, and paralysis of the third nerve. History of syphilitic attack two years before. Treated then by mercury. No history of secondary symptoms elicited by close questioning.

5. A case of osteocopic pains, paralysis of seventh nerve, followed by lichen and iritis, in a man undischarged from hospital for primary disease.

6. One of secondary syphilis, in which symptoms of intracranial inflammation appeared, followed by marked alteration of manner, loss of memory, dirty habits, &c.

7. Case in which there were—first, symptoms indicative of venous lymphatic absorption of pus, or some of the diseased products of secondary lesions; second, ocular disease, paralysis of third nerve, and other symptoms indicative of intracranial mischief.

The writer also adduces other cases, and makes some remarks upon the character and nature of these nerve lesions.

As illustrative of the long interval which sometimes ensues between an attack of primary and secondary symptoms, Dr. Marston adduces the following:—

8. Lepa psoriasis and syphilitic cachexia in a man who had not suffered from primary disease for nearly five years, who had been married for three, and whose offspring were healthy.

9. Syphilitic cachexia, &c., in an infant, who died of the disease; the father not having had primary symptoms for four years, and having been subsequently the parent of one healthy child.

The author then speaks of the occasional latency of the syphilitic element, until some other disease or impairment of health appears, and seems to act as an exciting cause to its manifestation. He

also remarks upon the modifications of diseases, or convalescence from them, that are sometimes, apparently, the result of a prior syphilitic infection.

In conclusion, Dr. Marston says the effect of the Mediterranean climate upon the syphilitic diathesis is very inimical, especially during the summer months.

SECT. II.—SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 97.—*On the Treatment of Damaged Brain from Mechanical Injury.*

By Mr. PAGET, Surgeon to St. Bartholomew's Hospital, &c.

(*Medical Times and Gazette*, Feb. 21, 1863.)

In a clinical lecture on a case of fracture of the base of the skull, among many excellent remarks, are these :—

“What is the treatment of fracture of the base of the skull? As regards the fracture, all we can do is to leave it alone. Occasionally, recovery will take place. As regards the damaged brain, do nothing but shut out all possible sources of mischief. For instance, keep away all possible causes of excitement. Just as we would keep a hurt joint quiet, so we ought to keep a hurt brain quiet. When the patient even looks or thinks, or is roused up, it is like moving the injured brain. In private practice it is difficult to keep the patient quiet, but in hospitals this can be more easily managed. The friends of such patients almost insist on something being done. This question of treatment may be illustrated by the way in which we treat an injury elsewhere, as, for instance, a bruised muscle. There is nothing to do but to keep it at rest. Would it occur to any one to bleed here? And yet it occurs to men of sense to bleed because a man has hurt his brain. What good would it do in injury to a muscle to give mercury? And yet it seems not unusual to give mercury in injuries to the head. Of course there are cases in which there are distinct indications for this kind of treatment. Its adoption as a matter of a routine in injuries to the head is to be deprecated. If (Mr. Paget said) the surgeon is not prepared to give mercury in bruised muscle, he has no ground whatever for giving it in injury to the brain. If, however (he continued), his patient had had symptoms of inflammation, he would proceed to treat it, but as yet he had had no symptoms of the kind. Delirium, convulsion, or other symptoms of cerebral disturbance, are not sufficient alone to establish the diagnosis of inflammation of the brain. The signs to be relied on are those that would indicate inflammation elsewhere—the pulse full and rapid, breathing rapid, skin hot, and the organs of sense acute, and the pupils acting more rapidly. If these symptoms are not present, we

ought not to bleed or give mercury, on account of delirium or convulsions, or other symptoms of cerebral disturbance.

"What we ought to do is, as before mentioned, to keep away from the patient all possible sources of excitement. We ought also most carefully to attend to the bowels and to the digestive organs. We should keep in mind that the patient can digest little, and, therefore, give but little, and that generally fluid. If the general health should flag, then we should give wine." * *

"In reference to treatment of inflammation following injuries, Mr. Paget said, that if the symptoms set in soon after—*e. g.*, within the first two or three days of an injury, or of an operation—we might bleed or depress the patient, or at least refrain from stimulating him. But when the symptoms set in later—*e. g.*, as in this case, on the eleventh day—then clearly the inflammation was an asthenic one. It was analogous to erysipelas setting in some days after an operation. If, after an amputation, we find tenderness and swelling of the stump within two days, we may treat it with depressing means, apply leeches or cold lotions, or, perhaps, rather, merely keep the patient quiet, and refrain from stimulants and full diet. But if on the eleventh day the stump were to inflame, we should treat the patient with good diet and stimulants. Just so in injuries to parts we cannot see. If after ten days signs of inflammation set in, we ought to give wine, just as we ought in erysipelas of the scalp setting in at that time. In fact, we ought to be guided by the time when the symptoms set in, as well as by the symptoms themselves. The man's breathing was more frequent, but it was more shallow; his pulse was quicker, but it was more feeble; so that everything indicated stimulants. That the treatment was unsuccessful does not necessarily show that it is wrong. In fractures of the base of the skull, if we save one in twenty, we should do very well indeed."

ART. 98.—*On the Water Cure in Puriform Ophthalmia.*

By M. CHASSAIGNAC, Surgeon to the Hôpital Lariboisière,
Paris.

(*Medical Circular*, April 15, 1863.)

M. Chassaignac published a paper on this subject in 1847, from which, and likewise from a subsequent communication of M. Velpeau to the Academy of Sciences on the same subject, it appears that this treatment was resorted to in 71 cases of puriform ophthalmia, and that in no instance was loss of sight observed, or spots on the surface of the cornea. This result is especially deserving of attention, inasmuch as at the Foundling Hospital, where M. Chassaignac carried on his researches, the cases of ophthalmia amounted to 250 or 300 in a total of 600 patients, and that omitting the instances in which death was induced by the disease, blindness followed in 8 or 10 of the children.

Since that period M. Chassaignac and several of his former

pupils, MM. Léon Rieux, A. Fournier, and Bricheteau, have frequently ascertained the undoubted efficacy of the ocular douche in the puriform ophthalmia of children and adults. Thus the *Bulletin de Thérapeutique* informs us that the method was recently resorted to by M. Bricheteau in thirty children, twenty of whom were more than two years old, and were perfectly cured in an average of ten days; the remaining ten were new-born infants, and here again the treatment was perfectly successful, vision being in all cases preserved in its integrity. At the Hospital of La Maternité, M. Danyau also follows this course of treatment with results deserving of notice.

"During the first quarter of the year 1862," says M. Nivert, an Interne of that Institution, in an appendix to M. Bricheteau's memoir, "600 births took place at La Maternité; 139 of the infants became affected with inflammation of the conjunctiva, the ophthalmia being puriform in 20 per cent. of the cases. Cold-water douches were prescribed in every instance, and in the severest only was a collyrium of nitrate of silver (5 gr. to the ounce) applied to the eyes. The intensity of the inflammation was not uniform. 39 were mild cases; in 61 the symptoms were more important, and 35 were of the most aggravated character.

"The following were the results attained:—72 children left the hospital in a perfectly satisfactory condition, after complete restoration of the eyes; 45, when discharged, still preserved a morbid degree of vascularity of the conjunctival surface, but were obviously doing well; 17 infants were removed by their mothers, during the progress of treatment, and before any material improvement had set in; 5 only died; they were, moreover, born prematurely; and were carried off by scleroma, or gastro-intestinal complications."

Whether, in these cases, the douche is merely a more powerful detergent than common injections, or, as M. Chassaignac believes, a local modifier of great energy, no doubt can be entertained as to its efficacy. We shall, therefore, briefly state the mode of operation recommended by M. Chassaignac.

The only instrument required is to be found in the poorest dwellings, and consists in the tin-funnel used for watering the floor. To the pipe is adapted an India-rubber tube, and the apparatus being previously filled with water, is suspended to the ceiling or to a nail immediately above the child's head. The stream of water is then directed to the bridge of the nose, and to the open eye, the strength of the douche being gradually increased. In hospital the reservoir is attached to the cross-bar above the bed, and contains four pints of water; the extremity of the tube is supplied with a cock, and the water runs through a waterproof cloth groove into a tub placed on the floor.

The douche should last from eight to twelve minutes, and be repeated as often as may be required by the severity of the symptoms, or the nature and abundance of the morbid secretions. M. Chassaignac uses water at the temperature of in-door fountains, or of wells in summer, and has also recourse to astringent collyria, two or three grains of nitrate of silver, or of sulphate of zinc, to the

ounce of water, for instance. In order to prevent agglutination of the eyelids, it is further desirable to lubricate the palpebral margin with some greasy substance, such as glycerine or cucumber cerate.

ART. 99.—*Diphtheria of the Conjunctiva.*

By Dr. JACOBSON.

(*Schmidt's Jahrbücher*, No. 2, 1862.)

This disease is said by Jacobson to be characterized by the transformation of the conjunctival tissue into a bloodless yellow grey or grey-white mass, which penetrates to various depths, and so has different appearances. The membranous formation offers nothing characteristic, but varies in individual cases. He describes the anatomy of a case ending in death, after measles, of a child ten months old. "In the microscopical investigation, the extended membrane appeared to be composed of an amorphous granular matrix, swelling up in acetic acid. In this matrix were distributed crowded large and small fat particles, and separate from them there were, moreover, irregular, angular, pale cellular formations without nucleus, some of the size of the pus-corpuscles, and some smaller. These might be considered as shrivelled pus-cells breaking up, and partly as detached fibrinous particles. The epithelium was wanting as far as the pseudo-membrane reached; it was partly preserved on the sclerotic conjunctiva, but irregularly disposed, dimmed, and very granular; the uppermost layers seemed to be wanting. The reticulation of connective-tissue corpuscles of the palpebral conjunctiva of the tarsus and of the submuscular connective-tissue was engaged in an extension, increasing in intensity from the deeper parts towards the surface; and, indeed, in the nearest surrounding parts of the smaller arteries and veins there always appeared the largest accumulation of cellular elements. Towards the surface one saw a dimming of the tissue by molecular fat particles, increasing progressively, and with it a decay of the cellular elements was connected. The most superficial layer appeared irregularly indented, formed of degenerating tissues infiltrated with granular matter. The cells of the Meibomian glands were almost wholly free of fat. On the transition fold and the sclerotic part of the conjunctiva the condition was nearly similar, only the degeneration on the surface did not appear to have yet taken place here, and therefore the epithelium was partly preserved. The epithelium of the cornea, as far as it was still maintained, showed a very granular appearance, hazy, and somewhat brownish in colour; the layers adjoining them were likewise dimmed with little fat particles, and the corneal corpuscles showed an increase of nuclei. In the ulcerating parts were found detached shreds of sloughing corneal lamellæ; the increase of nuclei and the granular infiltration extended rather deeper."

ART. 100.—*A Case of Intra-orbital Aneurism, cured by Ligation of the Carotid, after failure of Digital Pressure.*

By Mr. ERNEST HART, Surgeon to the West London Hospital.

(*Lancet*, March 15, 1862.)

The following case appears to be unique in the surgical literature of this subject. It is an instance of traumatic arterio-venous aneurism of the frontal branch of the ophthalmic developed within the orbit.

CASE.—Richard T——, a boy, aged 11, quarrelling in the street with another lad, received a blow from the forked end of an iron rib of a parasol at the inner angle of the left upper eyelid, about four years since. Rapid swelling of the eyelid followed. In a few minutes a considerable effusion of blood into the eyelid had occurred. It swelled to the size of a pigeon's egg, and became purple in colour, altogether obscuring the eyeball, since the lid could not be raised. The wound bled very freely. By the aid of pressure the bleeding was arrested. Subsequently, under the influence of time, cold lotions, and leeches, the swelling of the eyelid subsided, the eye remaining somewhat bloodshot. No ill effects were apparent.

The boy returned to school, and went on as usual. He used to complain afterwards of headache and singing in the ears; but it was not until the end of 1860 that the attention of his mother was drawn to a swelling in the site of the original wound of the lid, which beat with a perceptible pulsation. This gradually increased in size; the lid became protuberant, the eye projecting, and somewhat unduly vascular in its conjunctival surface. He was brought to my house on the 19th of January, 1861.

There was an aneurismal swelling at the inner angle of the orbit, just below the margin of the bone; it pulsated strongly. There was a perceptible thrill, and a very loud, whizzing bruit could be heard over all the left side of the head and temple. This bruit was continuous through both systole and diastole, but louder during diastole. Hence I inferred that there was a communication between the artery and vein, and that this was an arterio-venous aneurism, resulting from the transfixion of the frontal branch of the ophthalmic artery and its satellite vein by the forked end of the piece of iron. There was no scar perceptible where the external wound had been. The eyelid was swelled, and some dilated vessels coursed over the ocular conjunctiva of the globe, which was itself more prominent than that of the opposite side. The general health of the lad was good. He complained of a whizzing noise in the head, like that of a steam-engine at work, and he suffered from headache.

After causing my diagnosis to be verified by Mr. Erichsen, I resolved to apply digital pressure to the left carotid artery, with the view of obtaining a cure in the same way as we are in the habit of doing in treating aneurism in other parts of the body. Although entertaining the highest opinion of the capabilities of this always innocuous treatment, I had not any great hopes of its success in this case. That method is most effective to cure where a definite sac or diverticulum of the blood exists, and where, by retarding the circulation, the deposition of layers of fibrin on the roughened walls of the sac may be obtained. Here, however, there was but a small sac, situated between the artery and vein, and communicating with both. I had put the boy under chloroform to facilitate the perfect examination of the aneurismal disease; and by passing the finger between the eye and the

roof of the orbit, it could be felt that the projection at the angle was due to an enlarged and tortuous coil of artery, and that the artery was tortuous and dilated along the roof of the orbit. Here, then, was a case of arterio-venous aneurism, with the cirroid dilatation of the artery, and probably also of the vein, which commonly accompanies that disease. In such a case there is no roughened sac; the arterial blood passes by a smooth aperture into the vein: and these are conditions under which it is almost hopeless to expect that indirect pressure will effect a cure—almost hopeless, but not quite. I felt it right, therefore, to employ digital pressure on the carotid during three weeks, organizing for the purpose a staff of three persons, who maintained intermittent but complete pressure during several hours daily for that period. It was very easy to stop pulsation in the projecting tumour by pressure on the carotid, and the arrest of the pulsation always afforded the visible sign that the pressure was complete. In cases of true aneurism, some surgeons have thought it desirable that the pressure should be continuous and incomplete, allowing a small stream of blood to flow through the tumour and deposit its fibrin. In arterio-venous aneurism I have no doubt that it should be intermittent and complete; for here the object is, by stopping the flow of blood from the artery into the vein, to seal in the first instance the aperture of communication. If this complete pressure be not intermittent, faintness follows from the interference with the circulation of the brain. This boy always bore fifteen minutes of pressure very well. At first we all thought there was a marked improvement; and the boy said that he did not suffer so much in the head. However this may have been, at the end of three weeks there was much the same state of things as before. After some consideration I resolved to tie the common carotid artery, as offering the best means of cure.

In March, I operated, in the presence of Mr. Erichsen, Mr. T. Holmes, Mr. Walter J. Coulson, Dr. B. W. Richardson administering the chloroform. The operation lasted but a few minutes, and neither the vein nor nerve was seen. The ligature of the common carotid immediately caused the flattening of the pulsating tumour, and an entire cessation of its beat. On recovering from the chloroform, the boy was calm and sensible; there was no perceptible change of temperature in the heart, or in the eye, or in the neck. He slept well that night, and the most remarkable point in all the after-progress was the total absence of any symptoms which could have led to the supposition that the great artery of the neck had been tied, or that anything had been done to interfere with the circulation of the brain. The ligature fell on the eighth day. The patient was seen during his convalescence by Mr. Erichsen and Dr. Richardson. The only point to be noticed was, however, the absence of symptoms.

Looking to the frequency with which ligature of the carotid has produced the gravest symptoms of brain disorder, and the occasions on which it has been followed by softening of the brain, paralysis of the opposite side, and death, I was disposed to think—and it was also the impression of Mr. Erichsen—that this boy had a great advantage in that the collateral circulation had been developed previously by digital pressure, and the sudden diversion of the blood was hence attended with little or no shock to the brain. It was in this hope that I persevered in the use of digital pressure before the operation, after it was evident that it was not likely itself to effect a cure. I think it may perhaps be recommended that this preliminary measure should in future always be adopted, if only with that view.

The final result of the ligature has, in this case, been satisfactory. There is not, at the present time, any trace of the tumour, or pulsation at the angle of the eye. The sight is perfect. He has no pain or noises in

the head; he runs and plays about, and is clever at his book. There may be heard, by applying a stethoscope to the head, a whizzing bruit. This has been noticed in other cases after cure, but it has not affected the permanency of the cure. I attribute it to the retrogressive dilatation of the arteries which had occurred during the progress of the disease. This slightly cirroid condition will probably remain.

In the following table may be seen a list of the recorded cases of intra-orbital aneurisms treated by ligature of the primitive carotid, with the result:—

Author.	Date.	Result.
Travers	1804	Successful.
Dalrymple	1812	Successful.
Roux... ..	1829	Success incomplete.
Scott... ..	1834	Successful.
• Busk	1835	Successful.
Jobert	1839	Successful.
Velpeau	1839	Success partial.
Van Buren	Successful.
Herpin	1844	Successful.
Pétrequin	1845	Death.
Brainard	1852	Unsuccessful.
Curling	1854	Successful.
Nunneley	1852	Successful, sight gone.
Do.	1856	Successful.
Do.	1858	Death.
Do.	1859	Successful.
Bowman	1859	Death.
Do.	1860	Successful.
Syme... ..	1861	Successful.
Hart	1861	Successful.

It will thus be seen that the statistical results of the operation are not unfavourable, considering the magnitude of the vessel ligatured, and its importance to the nutrition of the brain. Out of the twenty cases which are here tabulated, in three cases the ligature of the common carotid was followed by death. In two the success was incomplete. In a sixth case the pulsation of the tumour was unabated by the operation. There remain fourteen successful cases out of twenty.

Mr. Hart attaches great importance to the treatment by compression of the artery leading to the aneurism, and he cites two cases in which this plan was carried out most successfully. If this plan has not been earlier applied to the treatment of orbital aneurisms, the explanation must probably be found in the fact that continued instrumental compression of the carotid artery in the neck is a proceeding of almost insurmountable difficulty. Since Professor Vauzetti, of Padua, has perfected the application of digital compression, this difficulty no longer exists. Digital pressure was maintained in the case of R. T— without inconvenience. It failed to effect a cure, but it probably modified favourably the condition of the tumour, and developed usefully the collateral circulation of the brain. In the first case, that of Maria O—, treated by Professor Gioppi, of Padua, the cure was effected by compression for a few hours in four days; in the other case, that of Catharina B—, treated by Dr. Scaramuzza, of Verona, by inter-

mittent compression occupying seven hours and twenty minutes in the course of eighteen days. It may perhaps serve to show how slowly the news of surgical progress may reach even the best-informed quarters, that no reference whatever is made to these cases in Mr. Nunneley's paper in 1859, nor does this surgeon mention the possibility of effecting a cure by this simple means.

CASE I.—Maria O—, aged 42, entered the clinique of the hospital of Padua, July 4th, 1856. She was of feeble constitution. Seventeen days previously, during an effort of childbirth, she felt as though her eye had started from the orbit. Four days afterwards the lids and globe of the eye were immovable, and there was complete blindness. On admission, the aspect of the patient was frightful. The eye lay motionless on the cheek; the pendulous lid was red and livid; the cornea infiltrated and opaque; blindness complete. There were pulsatory noises in the head, and the finger, when pressed back at the upper border of the orbit, felt an elastic aneurismal tumour thrusting forward the eye. Compression of the carotid very soon produced faintness, and Gioppi employed the method of Valsalva, fearing to use ligature. It failed. Digital compression was then employed for a minute or two at a time, suspending it when faintness threatened. This compression was effected with the finger by the patient herself, and some of the convalescents and others in the ward. The effect was of the happiest kind. On the following day there was already a diminution in the force of the pulsations; and at the end of the fourth day all pulsation had ceased. From that time all went well. Finally, the eye retreated within the orbit and sight returned, the patient remaining only somewhat myopic and with a dilated pupil.

CASE II.—Catharina B—, of Verona, a washerwoman, aged 49, small and weakly, was admitted into the eye ward of the civil hospital of Verona on April 4th, 1858. She was weak in health and subject to palpitation. A few days previously, after a violent access of fever, she felt an acute pain in the left orbit and ear; something seemed to give way in the orbit; the eye became enlarged, and the patient could not distinguish light from darkness. On her admission, the left eye projected entirely beyond the orbit; the lids did not cover the ball; the eye red; cornea dull. The patient could hardly discern the light. There was pulsation and thrill over the orbit and left temple. She was the subject of dilatation of the heart and of the arch of the aorta. Digital compression of the carotid was therefore employed here very cautiously, for not more than five minutes at a time. Summing up briefly the carefully recorded details of the case, it may be said that during the eighteen days that the treatment lasted, the total space of time during which compression was used amounted only to seven hours and twenty minutes. The eye had then entirely entered the orbit, and pulsation had ceased. The cure was complete.

ART. 101.—*On Inferior Section of the Cornea for the Extraction of Cataract.*

By Mr. ERNEST HART, Ophth. Surgeon to St. Mary's Hospital.

(*Lancet*, Oct. 18, 1862.)

In the operation for the extraction of cataract, Mr. Hart prefers the inferior section of the cornea to the superior section. "My

own experience," he says, "and the observation of a long series of cases in the practice of my friend, Mr. White Cooper, prompt me to speak much more favourably of the inferior section than do some of our classical writers on ophthalmic surgery. From the results of a large number of cases of extraction, in rather more than half of which I have operated by the inferior section, I have great reason to be satisfied with that method. In a number of other cases which I have had opportunities of observing, the result has been as good.

"It has been objected to the inferior section that the edge of the lower lid is likely to become engaged in the wound, and so to retard union; and that by its position, being bathed in the tears of the inferior cul-de-sac of the mucous membrane, the healing of the cicatrix must suffer from that contact. I believe both these objections to be partly fanciful, and that they are not fully borne out in practice. I have never seen more rapid union than in the cases of inferior section, and the excellence of the ultimate result is greatly aided by the more favourable conditions which it offers for executing the operation to perfection. In employing the superior section, there are difficulties inherent to that method. These occur especially in the second and third stages of the operation. After the section has been made and the eye released, the ball turns upward under the lid so as to bury the incision, and the introduction of the cystitome, the expulsion of the lens, and the perfect clearing of the pupil are all infinitely more difficult than in the lower section. Practice teaches how to overcome these difficulties; but I am persuaded that the greater facility with which the pupil may be cleared and the parts adjusted has the effect of producing more perfect results from that operation. The accidents of operation—and in these I include wounding the iris, effusion of blood into the anterior chamber, difficulty in extracting the crystalline lens, incomplete incision of the capsule, declension of the lens into the vitreous humour—may be almost wholly excluded from operation by the inferior section. By my own experience I am led to similar conclusions in respect to prolapse of the iris and synechia—two of the most troublesome accidents so far as the after-consequences are concerned. In one or two patients I have operated with the same degree of care by the inferior section on the one eye and by the superior section on the other, and the result has confirmed a preference for the former method. Thus, in Catharine B., who was lately under operation at the West London Hospital, the result on the left eye (inferior keratotomy) was perfect; in the right (superior keratotomy) the iris is adherent to the corneal cicatrix. The same has happened in two other of my cases lately. I know no more beautiful, simple, and successful operation than extraction by the lower flap."

ART. 102.—*On Paracentesis of the Cornea.*

By Mr. GEORGE LAWSON.

(Royal London Ophthalmic Hospital Reports, Jan. 1862.)

In this article Mr. Lawson relates some cases illustrative of the advantages of tapping the anterior chamber of the eye in cases of sloughing ulcers of the cornea, or of ulcers which refuse to heal under other modes of treatment, in cases of onyx, or of ophthalmitis with hypopion.

"The operation," he remarks, "is a simple one, and best performed by the manner usually adopted by the surgeons at Moorfields—viz., by passing a broad needle through the cornea at its lower margin, keeping the point well forward towards the cornea, to avoid wounding the lens, and then suddenly turning it on its edge so as to allow the aqueous to run off, and rapidly withdrawing it as soon as the iris approaches the cornea.

"The indications which call for this line of treatment may be briefly stated.

"1st. Increased tension of the globe. The eye, in any of the above-mentioned cases, may have its tension slightly increased, and this seems, in a great measure, to depend on an increased secretion of the aqueous, for the anterior chamber becomes deepened; and this is specially observable when contrasted with that of the other eye, and the iris, instead of presenting a plane surface, slopes backwards.

"2nd. Deep ulcers which threaten to perforate the cornea will often rapidly assume a healthy action after the tension of the cornea has been diminished by letting off the aqueous humour; and sloughing ulcers will, under the same treatment, derive similar benefit.

"That this benefit is often only transitory is true, but the operation is so simple, that it may be repeated an indefinite number of times, if the patient after each derives relief.

"3rd. In cases of onyx, or pus between the laminae of the cornea, the relief of the tense state of the cornea promotes absorption and relieves pain, and so places the eye in a favourable condition for complete recovery. An onyx which threatens to burst backwards can be most safely combated by tapping the anterior chamber of the eye in addition to the use of other remedies.

"4th. This operation relieves pain, and, if carefully and properly performed, can do no harm. The relief of pain is so remarkable, that patients, on their next visit to the hospital, will, unasked for, relate the great benefit they derived from what had been done to the eye; but at the same time they will often state that after about twenty-four hours the pain was nearly as bad as ever. The truth being, that the aqueous had become completely restored, and that the tension which called for the first tapping was as great as ever.

"The operation in such cases should be repeated."

ART. 103.—*A Case in which extensive Epithelial Cancer was removed from the Orbit.*

By Mr. MOORE, Surgeon to the Middlesex Hospital, &c.

(*British Medical Journal*, Aug. 23, 1862.)

The patient upon whom this operation was performed was present at the meeting of the British Medical Association, at which the account of her case was read. Mr. Moore's object in operating was to lengthen life and to preserve the sight of the left eye, and the result fully justifies the proceeding.

CASE.—The patient is 71 years of age, in good general health, and as free from feebleness as could be expected in a woman of her age after the recent removal of a serious disease.

Upon her admission into one of the cancer wards of the Middlesex Hospital, seven months ago, she presented a deep ulcerated excavation between the globe of the eye and the bones at the inner side of the right orbit. The lids had been detached by the ulceration from their internal connexions, and partly destroyed. What remained of them was fixed in the solid outer wall of the ulcer. The eye was shrunken and sightless, having apparently been perforated through the sclerotic by the ulcer, and evacuated of a part of its humours. The margins of the ulcer reached the upper and nearly the lower edges of the orbit, and terminated about the mesial line upon the bony nasal ridge. From the level of its edge, the ulcerated chasm had a depth of nearly an inch; its breadth was half as much, and it passed straight backward close to the lachrymal and adjoining bones; none of which, however, were bare.

The disease presented the ordinary characters of epithelial cancer. Dry scab, or pale pink nodules, which could not be called granulations, covered the ulcerated surface. The edge was sinuous, and was everywhere formed by a firm, solid, new material, which increased the soft skin to a thickness of about one-eighth of an inch. A tubercle of similar substance and of about the size of a large pea existed also in the upper eyelid. There was no glandular disease.

The growth had commenced at the inner part of the lids about three years and a half previously to her admission. It had steadily increased, and, having caused her little pain, had been subjected to no treatment.

For two months, various local applications were made to the ulcer, and to the parts adjoining it, but with no definite advantage. The disease continued to increase. She was therefore ordered minute doses of Donovan's solution of mercury, iodine, and arsenic, and she continued to take that remedy from February 20 to April 14. Although there did at first appear to be less thickening around the ulcer, yet on the whole period of this treatment the disease decidedly increased. A greater extent of the lids disappeared, and the solid deposition which presaged ulceration advanced over the forehead and cheek. But the most serious change in the disease was its progression inward. It crossed the bridge of the nose, and reached within three-eighths of an inch of the inner commissure of the healthy eyelids. This advance, as was evident, threatened the destruction of the remaining eye.

Had the patient been unhealthy, and likely to live but a short time, it might have been little worth considering whether she should undergo any operation for the sake of preserving her sight. But in the opposite circum-

stances, when the general health was sound, and the prospect of life good, I thought it important to interrupt the progress of the disease, at least toward the sound eye; even though I should be compelled to leave it to its natural course in other directions. The plan adopted was as follows:—

On the 23rd of April, nine days after she had ceased to take the Donovan's solution, she was put under the influence of chloroform, and I made an incision in the healthy skin immediately surrounding the whole disease. The incision was carried beyond the margin of the orbit, both on the forehead and cheek, but descended on the outer side through the external remnant of each eyelid. On the inner side, it included all the structures between the lower part of the forehead above and the higher part of the nasal cartilages below, and swept round the left side of the nasal bones at the distance of a quarter of an inch from the left tendo oculi.

The parts included within this irregular incision were severed from the bones, and with the globe, the ulcer, and its walls, were excised. The bleeding mostly stopped under pressure; in parts where it continued, it was arrested by the actual cautery and the perchloride of iron.

Upon examining the cavity, it appeared that the disease had not been completely extirpated by the knife. The nasal, lachrymal, and ethmoid bones, near which the growth had first sprung, were perforated in several spots, and prolongations of cancerous disease were found in the apertures. Neither the frontal nor the cheek bones, moreover, were safely divested of the morbid growth, although none of it could be discerned upon them or on their periosteum. Over the whole of the surfaces which could be suspected of being diseased, I accordingly laid cotton-wool spread with a paste of the chloride of zinc, and took precaution against any of the paste itself, or diluted with blood and serum, passing into the left eye. Before the patient awoke from the chloroform, I had a minute quantity of a saturated solution of morphia injected under the skin of the arm.

The patient was taken to bed, and slept soundly for several hours. On awaking, she had no pain in the wound. No bleeding took place. The zinc, blood, and wool hardened together, and became an inodorous mass, which came away only with the subsequent slough.

On the 24th, she had one epileptiform fit. She lost consciousness for about ten minutes, struggled, foamed at the mouth, and bit her tongue. Upon recovering, she was quite sensible, spoke freely, and complained of pain in the loins. Pulse 96.

The pain in the loins was her only complaint from that time forward. It prevented her from moving, kept her awake at night, and was the subject of loud and seemingly exaggerated complaint whenever she was spoken to. Of her head and of the wound she never complained. Her urine was found healthy, both in appearance and in chemical examination. The seat of the pain shifted from day to day, and it appeared to be muscular. In a few days it was found not to interfere with her sleeping or her appetite; and in the course of a month, it slowly declined, though she continued for all that time vehemently to complain of it.

The entire slough was loosened in about five weeks from the time of the operation, and I had no difficulty in lifting it off. It included almost all the soft contents of the orbit which had not been removed by the knife, and a circle of bone, of greater or less thickness, from the whole circumference of the orbit. A part of the orbital plate of the frontal bone, as large as a shilling, had been detached; and rather more of the floor of the orbit, together with both the nasal bones, and some of the septum of the nose, and of the thin osseous plates which had enclosed the air-cells. No trace of the original disease appeared in any part of the wide excavation disclosed by the removal of the slough.

The cicatrization of the greater part of the wound was rapid, as it started forth, not only from the skin, but also from many edges of exposed mucous membrane. Only at the roof of the orbit was the healing at all delayed.

The gap left by the operation is of considerable size, but less deep than it would have been but for the loss of the edge of the orbit. It reaches laterally from the ascending plate of the left superior maxillary bone to the right temple, and from the depressed superciliary ridge downwards to about the middle of the body of the right superior maxillary bone and the nasal cartilages. Exposed in the gap are the antrum, the orbital plate of the sphenoid, the pulsating cranial wall in the place of the defective orbital plate of the frontal bone, some ethmoidal air-cells, parts of the three right turbinated bones, and, through the aperture in the septum, some of the inferior turbinated bone of the left side.

The cicatrix and the exposed parts are little sensitive, and not at all painful, when touched, and the local applications rarely even make her sneeze. The forehead, malar region and ala of the nose, and upper part of the right cheek, are not sensitive; but the upper lip readily perceives a touch. The superior maxillary nerve is therefore partly destroyed; the nasal, malar, and frontal wholly.

ART. 104.—*On the Calabar Bean as a New Agent in
Ophthalmic Medicine.*

By Dr. D. ARGYLL ROBERTSON.

(*Edinburgh Medical Journal*, March, 1863.)

“For more than a year past,” writes Dr. Robertson, “I have recognised the numerous advantages that would flow from the discovery of a substance which, when applied to the conjunctiva, should produce effects exactly opposite to those well known to result from belladonna or atropine; which should stimulate the muscle of accommodation and the sphincter pupillæ as the above-named remedies paralyse them. With the view of discovering such an agent, I endeavoured to ascertain from experiments of my own, and from the writings of previous observers, whether any of the common vegetable principles possessed this property. These investigations were, however, productive of no satisfactory results, until my friend Dr. Fraser informed me that he had seen contraction of the pupil result from the local application of an extract of the ordeal bean of Calabar. I resolved to investigate the action of this substance, and, above all, to ascertain whether it exerted any influence on the accommodation of the eye. With some difficulty I got a few Calabar beans, and, with the view of obtaining their active principles in a convenient form, prepared from them three extracts, of varying strengths, in the following manner:—The weakest of the three was made by thoroughly digesting gr. xxx of the powdered Calabar bean in alcohol, carefully evaporating the strained fluid to dryness, and then adding to the residue ʒj of water. This formed a muddy solution, of a light reddish-brown tint. In strength, one minim of this solution corresponded to about half a grain of the bean. The second extract was formed by evapo-

rating a portion of the first to about a quarter of its volume; so that one minim of this extract corresponded to about two grains of the bean. The third extract was the strongest, and was prepared like the first, except that the proportions differed; so that one minim of it in strength corresponded to four grains of the bean. With these solutions I then proceeded to perform the following experiments—in which I had the able assistance of Dr. Grainger Stewart—with the intention of elucidating the exact effects the Calabar bean is capable of producing on the eye."

These several experiments are described, and the conclusions from them are stated as follows:—"These experiments prove that the local application of the Calabar bean to the eye induces—first, a condition of shortsightedness. That this is present, and the cause of the indistinctness of distant vision, cannot be doubted, as it is relieved by the use of concave glasses. The fact that objects appear larger and nearer than natural may be attributed to the induced myopia. And, second, it occasions contraction of the pupil, and sympathetically dilatation of the pupil of the other eye. We further observe that atropine possesses the power of counteracting its effects, and, *vice versa*, that it is capable of overcoming the effects produced by atropine. The first symptom noticed is dimness of distant vision, and shortly after, the pupil becomes contracted; the symptoms also subside in the same order—first the derangement of accommodation, and then the affection of the pupil."

ART. 105.—*A Case of Caries of the Internal Ear, in which a Sequestrum was Extracted which included the Semicircular Canals and Cochlea.*

By Dr. C. R. AGNEW, Surgeon to the New York
Eye Infirmary, &c.

(*American Medical Times*, April 16, 1863.)

CASE.—W. C., æt. 38, had suffered from otorrhœa from the right ear for the greater part of thirty-two years. The origin of the disease was obscure, the patient rather vaguely attributing it to a fall. Considerable sense of hearing remained until three years before the case came under my observation, at which time an exacerbation of the aural inflammation, accompanied by prolonged and excessive pain deep in the ear and through the neighbouring parts of the head, terminated in total loss of hearing in the affected organ, and paralysis of the corresponding portio-dura of the seventh pair. Several times during the progress of the disease, granulations sprouting from the depths of the external ear outcropped at the meatus, and were removed by torsion.

The patient came under my observation for the first time on the 16th of April, 1862, presenting evidences of great suffering and debility. He had suffered greatly for months from gnawing pain in the ear and head, insomnia, loss of appetite, and dizziness. An examination of the diseased ear was effected with great difficulty, on account of its excessive tenderness. The concha, swollen and inflamed, was elevated by a dense inflammatory tumour.

faction circumscribing the external meatus, extending backwards over the mastoid process, and forwards along the zygoma. Projecting from the meatus was a large pear-shaped "polypus" of a dense fibrinous character, bathed by a constant flow of stinking pus. Desiring to get to the bottom of the case, I placed the patient under chloroform, and removed the polypoid mass by means of a wire snare. In attempting to push the wire snare into the depths of the meatus I encountered a solid obstacle in the region of the middle ear, which subsequently proved to be the sequestrum. The calibre of the external meatus had become greatly reduced by boggy swelling of its soft parts, so that I was compelled to make as free an incision as possible to enable me to reach the sequestrum with a pair of small dressing forceps. Having got the body in the grasp of the forceps a slight rocking motion with traction enabled me to extract it.

The sequestrum included the wreck of the labyrinth. The cochlea was laid open by caries, and two of the semicircular canals were seen in part. The loss of hearing and the paralysis of the seventh pair were explained.

After the operation the patient rapidly regained his health, the discharge from the meatus ceased, and by the 3rd of January, 1863, the external meatus had become entirely closed by cicatrization. The paralysis of the portio-dura still remains.

ART. 106.—*Case of Sphacelus of the Tongue.*

By Mr. AUGUSTIN PRITCHARD, of Clifton.

(*British Medical Journal*, Nov. 8, 1862.)

CASE.—George Nichols, aged 50, a coal-miner from Monmouthshire, was admitted as an in-patient on the 28th of last July. Ten months before that time, a small pimple appeared on the left side of his tongue, without any very obvious cause. He himself seemed inclined to assign it to his habit of smoking, which he had carried to excess, having, as he said, always held a pipe between his teeth, perpetually puffing whilst at his work underground. He was a sober man, and never had syphilis.

The inflamed spot became worse, and so painful that he was unable to eat on that side of his mouth; and the disease increased so much for the next month, during which his tongue was daily touched with nitrate of silver, that he was unable to continue his occupation.

When I first saw him, his tongue was red, very hard, and immovably fixed in its place behind the teeth; but it was not at all swollen, nor ulcerated. His speech was very imperfect.

I was much at a loss to determine the nature of the disease; for, although the tongue was of a scirrhus hardness, the progress of the malady and the patient's freedom from any other signs of cancer or of glandular disease inclined me to consider it innocent, and not malignant induration.

I ordered him some iodide of potassium internally, good food, and a solution of the chlorate of potash with a small quantity of the tincture of the sesquichloride of iron for his mouth. There was no change in his state for a few days, but then he began to complain of increasing difficulty of swallowing; and the end of his tongue became very soft and dark, and ultimately it turned quite black. Hæmorrhage to a small extent, daily recurring, came on; and with the blood, which was continually dribbling from his mouth, was mixed some black and excessively fœtid matter.

He was directed to wash out his mouth continually with Condy's fluid diluted; and a little stimulus was given to him. The liquor carbonis detergens was also used, as well as many of the chlorides.

The disease spread until the whole of his tongue became a mass of soft and black putrifying tissue, which no disinfectant would touch. As might be supposed, his difficulty of swallowing became much greater, and he could get down but a few drops at a time ; but he did not seem to be particularly affected by the mixture of the teaspoonful of wine and water or beef-tea with the putrid liquid oozing from the mass of sphacelus which nearly filled his mouth.

The poor fellow, throughout the progress of this horrible disease, always looked cheerful and hopeful and contented ; and although his speech was at this period very muffled and indistinct, we could understand him to say that, if he could swallow, he would be better.

The process of starvation was rapid. He became excessively thin and haggard, and cold in his extremities ; and by the time the black slough began to separate at the apex of the tongue, he became pulseless.

The case appeared quite hopeless. There was every prospect of his immediate death from starvation ; and, besides, there was the risk of the extension of the disease to the pharynx and glottis. To relieve his sinking state, I ordered an injection of strong beef-tea and brandy twice daily, as he was quite unable to swallow ; and after the third injection his pulse returned.

The ragged and putrid black mass which represented his tongue was at this period coming away in shreds ; and, after about three weeks from the beginning of the mortification, the last portion separated, leaving a tolerably clean surface covered with flabby granulations, bounded by an irregular edge posteriorly, just high enough to hide the epiglottis.

He now regained the power of swallowing fluids, but in a peculiar way. He could drink ; but the last portion of the fluid, which filled his mouth, was always rejected, as he had no power of bringing it back within the scope of the pharyngeal muscle. His speech, which had been unintelligible, now became of service to him ; and at the present time (Sept. 25th) perhaps more than a fortnight after he passed through that terrible crisis, he can make himself easily understood.

The emaciation was so complete that he was quite haggard, his eyes being sunk in the orbits ; and when he could swallow, and the separation of the slough gave him the feeling of returning health, his craving for the fat of meat was most remarkable. Large pieces of the fat of pork and roast mutton were sent up to him at dinner-time ; and he masticated and squeezed out the softer parts with his teeth, leaving but shreds of hard tissue behind. He could swallow no solid, and particularly disliked the lean part of the meat.

At the present time the mouth is clean, and the floor is healing over. He can talk tolerably well, but can only swallow while he is lying down. He has regained his strength so far that he is able to walk about with ease.

Since the date when this report was read, the patient has still further improved in strength, and has returned to his home.

ART. 107.—*On a Peculiar Form of Disease of the Jaw in an Aged Patient.*

By Mr. FERGUSSON, Surgeon to King's College Hospital, &c.
(*Lancet*, Sept. 6, 1862.)

What Mr. Fergusson says upon this subject is reported as "clinical remarks" in the *Mirror of the Practice of Medicine and Surgery in the Hospitals of London* :—

"Familiar as he was with diseases of the jaw, and he had seen as many examples as most surgeons, here was an instance, Mr. Fergusson remarked, of disease he had never seen before or read of. About the middle of June the patient had called upon him with a letter from a friend. He noticed something wrong with his jaw, and on looking into his mouth, he asked if he had a bit of potato in it. To his astonishment he found it was a growth upon the jaw. Some surgeons would call it fibrous; but it was a form of disease which he had never met with previously in this or any other part of the body. It looked like vegetable matter, or greatly elongated papillæ. He could not undertake to give it a name. It was something like malignant disease; and the question arose as to what ought to be done.

"The friend who wrote to him (Mr. Fergusson) had operated several times. It would, perhaps, have been better to have removed the whole of the jaw; but he did not like to submit so old a patient to such an operation, for his age was eighty years. He selected a milder method, that of cutting the disease out instead of making a large wound in the cheek. He removed on this occasion (June 21st) probably the greater part of it, together with its base. It certainly was not unlike medullary disease. He thought he had succeeded in taking away the whole of it. If there should be any left, he expected to remove or destroy it by means of chloride of zinc. We shall see, Mr. Fergusson observed in conclusion, when the granulations spring up, the process the disease may take; and we must look upon the present case more as an instance of the curiosities of pathology than of surgery.

"It may be remarked, that the disease was confined to the right side of the lower jaw, and looked like meat that had been macerated for a long time, and had become bleached of a pinkish white colour.

"On the 12th of July it became necessary to repeat the operation; for although but three weeks had elapsed since the last occasion of removal, the tumour had grown very rapidly, and in general characters resembled its predecessor. It was cut away chiefly by means of curved forceps, and portions of it were scraped from the bone. With regard to the last, very little of it was left, and Mr. Fergusson mentioned that it was necessary to proceed with caution in such an old patient.

"After remaining about another fortnight in the hospital, the patient left for the country. Up to this time there had been no further recurrence. The cicatrix was, however, touched with the chloride of zinc."

ART. 108.—*Osteoplastic Resection of the Jaws.*

By Dr. BILLROTH, of Zurich.

(*Langenbeck's Archiv*, vol. ii. p. 651; and *Medico-Chir. Review*, Jan. 1863.)

Our readers are no doubt acquainted with the operation which Langenbeck has proposed and successfully carried out on the upper jaw, for the removal of tumours situated behind that bone. It consists in separating the superior maxilla from its connexions suffi-

ciently to allow of its dislocation, so as to penetrate behind it and remove the tumour without so far separating the bone from the surrounding soft parts as to destroy its vitality. The tumour being removed, the dislocated bone is replaced, adheres like any other compound fracture, and the patient recovers without any loss of substance in the face. Dr. Billroth, of Zurich, has applied the same principle to the lower jaw in two cases of extensive tumours situated beneath it. He divides the lower lip, or cheek, down to the bone, in one or two places, as may be necessary, saws through the bone with a chain-saw at the same place, separates it from the soft parts on its inner surface, and displaces the portions of the jaw (still adhering to the external soft parts) sufficiently to obtain access to the interior of the mouth and side of the pharynx and larynx. The operation having been completed, he re-unites the separated portions of bone by metallic sutures introduced through holes bored in them. Of the two operations performed on this plan, which he describes in the present paper, one at any rate seems to have been (temporarily at least) successful, and the bone re-united without any deformity, except from the cicatrices. In the other case the patient died of the direct effects of the operation, the pneumogastric nerve having been divided.

ART. 109.—*Apparatus for the purpose of Remedying Defects in the Parts about the Mouth.*

By M. PRÉTERRE.

(Jurors' Report of the International Exhibition for 1862.)

M. Préterre exhibits, under the title "Prothesis," a numerous collection of apparatus for the purpose of remedying defects in the parts about the mouth—clefts in the hard and soft palate, both congenital and accidental, and losses of portions of the maxillary bones from various causes. The different apparatus are susceptible of the following classification:—(a) *Plain Obturators for Perforations of the Hard Palate.* There is nothing new in these, but they are made on the best principle, simply spanning across the perforation and avoiding the injurious plug in the nasal cavity which is too often adopted. (b) *Various Combinations of Obturator and Artificial Velum* for cases of cleft both of hard and soft palate. These are very diverse, to meet the particular losses which have required restoration. M. Préterre has applied spiral springs to keep the artificial soft palate in proper apposition; and, though the method is not new, it has been ingeniously modified to suit individual cases. One plan of forming artificial hard and soft palate in a continuous structure appears to be quite original. This object has been accomplished by employing caoutchouc mixed with different proportions of sulphur, that forming the hard palate becoming indurated by heat, while that which replaces the wanting soft palate remains pliable and elastic. (c) *Masses of Vulcanite bearing Teeth to supply con-*

siderable losses of Maxillary bone. These are not new, but several are exhibited of excellent workmanship; and M. Préterre has applied in some cases the combination of soft and indurated caoutchouc; the latter for bearing false teeth, the former for contact with recent and tender cicatrices. (d) *Artificial Ginglymus* and side supports, in cases where the temporo-maxillary articulation has been injured, the ascending ramus of the lower jaw removed, or the integrity of the inferior maxillary arch destroyed. M. Préterre has, by the appliances here named, endeavoured to arrest the lateral traction of the pterygoid muscles, which is so troublesome, where their normal antagonism is suspended through a lesion of the arch of the lower jaw, and at the same time to supply the function of a new joint. The hinge employed is not absolutely new, though (as far as the Jury are aware) there is no published account of it; but the side processes extending from one jaw to the other, thus preventing lateral displacement, are, the Jury believe, entirely original. The Jury are satisfied that the different forms of apparatus displayed by M. Préterre have been devised to meet the exigencies of real cases, and are not merely theoretical combinations for possible contingencies. Indeed, they have the assurance of M. Nélaton that many of them have been inspected by himself, and that they have satisfactorily answered the purposes for which they were intended. M. Préterre has thus, in the opinion of the Jury, made real and substantial advancement in the difficult department of dental surgery to which he has devoted himself.

ART. 110.—*Report on Gangrene of the Throat, a Disease first observed at the United States General Hospital, Department of the Gulf.*

By Dr. RUFUS KING BROWNE.

(*American Medical Times*, Nov. 1, 1862.)

"The disease I describe," says Dr. Browne, "is a new form of hospital gangrene—a new disease, if what varies so widely from ordinary hospital gangrene, but is of the same type, is entitled to be so called. I designate it gangrene of the throat. It was found to involve the root of the tongue, the ventricles, and the cartilage of the larynx. The cases presented no symptoms which indicated the progress of the ravage, except in one out of fourteen instances, in which, during the last half hour of life, dyspnœa was very marked. In one case, the connexion of the arytenoid and thyroid cartilages was destroyed, with the outer part of the tissues covering the side of the latter. In all the others of that number, none, except two, which presented moderate factor of the breath, presented any peculiar sign of the commencement or progress of the disease. In one or two instances of the above number, the autopsy disclosed slight cedema of the glottis, and equally slight serous infiltration of the loose tissues adjoining the larynx. None of them presented any symptoms of inflammation of the throat; neither pain, sense of distress, nor constriction. The disease did not present any of the characters of ulceration, nor assimilate any of its characteristics of heat or

morbid redness. Unlike hospital gangrene, it occurs without febrile symptoms or tumefaction, or the characteristic edging of that form of ulceration.

"When found after death, the destroyed part has the colour of gangrened pulmonary tissue, though devoid of the very strong fœtor of the latter.

"The exact character of the first stage of the disease, being not open to inspection, is undetermined. In the last of the autopsies in which it was discovered, I sought to ascertain this, but ineffectually. The œsophagus was not injured by the disease, nor did it appear on the external surface of the body. Neither did it appear in any cases of decided scorbutus; yet in one case it did in the alveoli of the lower jaw, which presented likewise a scorbutic appearance.

"In the foregoing I describe as thoroughly as now practicable this species of gangrene. The disease was found in eleven of the forty autopsies made by my own hands, expertly assisted by Dr. Clary, in the fifteen days ending with the 13th September. It may be a question whether the disease is not as frequent elsewhere in military hospitals, and the ordinary amount of examination in autopsies does not disclose it. Ordinarily, although the lungs, trachea, and bronchi are examined, the throat is not, except in cases where throat symptoms are manifest before death. Upon this turns the question of the frequency of gangrene of the throat. I trust this point may be decided by the concurrent search of other surgeons. It may already have been elsewhere detected. Of the cause or condition of its appearance, I am without anything but surmise. It was found in debilitated and greatly reduced patients, but of the nine hundred in this immense hospital, at the time the autopsy which first disclosed the disease was made, none were otherwise. Including these, there had been previously collected at once in its wards and spacious galleries upwards of twelve hundred under my charge, but such of these as presented no specific ailment were sent to other quarters.

"Among the number which remained, there were but four wounded who did not die. The occurrence of the disease, therefore, did not resemble the conditions of hospital gangrene. As soon as its presence was discovered by autopsy, I caused an inspection of the throats of all the patients, and isolated those suspected. Of these one died; not, however, of this disease, for his case was one of ulceration of the throat, with the ordinary symptoms of distress, circumscribed redness, and purulent formation. Other cases of ulceration were radically improved by topical treatment with sol. argenti nitratis.

"In one instance only did such inspection of the throat reveal gangrene. Here it had invaded the space between the pillars of the fauces. It was treated vigorously with a strong solution of nitrate of silver. The only complaint this patient made was, that his mouth was sore. He died. In the most recent autopsies the disease does not appear.

"I describe this disease so soon after my experience of it, in the earnest desire that others may guard against its being overlooked. I know but little respecting it; I have simply observed—

"*First.* That it appeared among the sick and debilitated patients, no wounded, nearly all of whom had been reduced by that most *deadening* of all diseases, the Mississippi or marsh fever. Many of these had long been afflicted with chronic and incurable diarrhœa. They were weak and feeble. Precisely similar were some of these cases in the manner of their disease and death, to those named by your correspondent, Dr. Rawson. Perhaps, however, otherwise than Dr. Rawson thinks, debility, and not scurvy, is the cause of death in these cases, for many undoubtedly scorbutic and debilitated recuperate, while others debilitated and not obviously scorbutic die. They

would rise, walk feebly but steadily along, return to bed, and immediately die. The autopsy revealed no lesion or organic disease. In vain I searched brain, heart, lungs, and the other organs, for some of the familiar causes of sudden death. I spent in the search every moment of spare time. In but four cases did I discover any of these causes. It was in the midst of this ineffectual search for its special purpose that I at length noticed, issuing from the mouth of a dying man, a little foul-looking and slimy fluid, of a peculiar cadaverous odour. He had never complained, so far as the attending physician was aware, of any throat difficulty; but, on autopsy, I explored the throat and found the disease I describe.

"*Secondly.* So far as I have discovered, this disease attacks the throat and no other organ. It has appeared only in the mucous membrane. I sought, but failed in every instance, to find it in the same case with gangrene of the lung. Nor have I found it in the same case with scorbutus or the ordinary hospital gangrene.

"*Thirdly.* Not a single one of the conditions known as productive of hospital gangrene existed during its production here. The wards of the hospital are very lofty, and its spacious and equally lofty galleries and corridors will entertain in the most salutary manner twenty-five hundred patients, but at no time was there more than twelve hundred and fifty. There is no part of these secluded from a free sweep of fresh air."

In all the instances examined the legs were cedematous, and there was a considerable quantity of serous fluid in the peritoneum and pleura. In all instances but three there was, in addition, a striking flabbiness of the heart.

"It is thus," Dr. Browne concludes, "the northern soldier, to whom the environment of a northern atmosphere is *literally a part of his being*, yields his life in defence of liberty.

"People do not 'stand' upon their muscle here, *they sit and lounge*. Find a man anywhere, and if there is a resting-place of only the dry ground he is '*down upon it.*' Soldiers do not carry their knapsacks, the quartermaster does it. Nor do they stand guard with their muskets. They sit guard in shirt sleeves and detached bayonet.

"These are the conditions, it appears to me, which hold a direct relation to gangrene of the throat—a non-inflammatory—a non-febrile, destructive disease."

ART. 111.—*Acute Inflammation of the Vocal Cords, seen by the Laryngoscope, producing complete Aphonia, and rapidly yielding to Topical Treatment.*

By Dr. GEORGE D. GIBB, Physician to the West London Hospital.

(*Lancet*, Sept. 27, 1862.)

CASE.—Mary H—, aged thirty-four, married, and mother of one child born sixteen years ago, was admitted as an out-patient Aug. 11th, 1862, for aphonia. Last winter she had suffered from an attack of bronchitis, from which she recovered, but occasionally she had coughed up to the present time. A week ago she complained of rheumatic pains, for which she took medicine, and almost immediately after her voice completely "went away," for she could not speak at all. This she thought was the result of taking

cold after the medicine, for she sits in draughts. She is subject to burning heats.

On examination the aphonia was found to be complete; she could utter a whisper, but so low and faint that it was with considerable difficulty she could make herself understood. She had pain at the back of the nose, and also, externally, in the upper part of the thyroid cartilage, to which she pointed with her finger, corresponding to the hollow behind the pomum Adami. On inspection with the laryngoscope, which was performed without any difficulty or resistance, the mucous membrane covering the lower vocal cords was seen slightly tumefied, and of a bright crimson-red colour. The bright redness seemed to be confined to this part of the larynx, for other parts were of a light pink. The action of the cords was limited, for they divaricated but slightly on forcible breathing; she could utter no audible sound, such as "ah," or the letter "a". As there was no solution of the nitrate of silver of the required strength already prepared, nor a proper instrument handy for its application, local treatment was deferred till the next visit. She was ordered, however, a blister to the neck, an aperient of rhubarb and calomel, and a mixture of iodide of potassium, sal volatile, and tincture of sanguinaria in camphor mixture, three times a day.

Aug. 14th.—No improvement in the aphonia; she eats but little, and swallowing is painful; the blister had risen well, but the neck was very tender; the inflammation has not extended, and the parts of the larynx involved present the same appearances as before. A solution of nitrate of silver (two scruples to an ounce of water) was applied directly to the bottom of the larynx, by means of a curved camel's-hair brush. This was followed by comparatively little spasm, and a very slight amount of dyspnoea—indeed less than was anticipated. Her previous mixture was omitted, and in its place one ordered containing small doses of tartar emetic, nitrate of potass, and acetate of ammonia in water.

This patient did not return to the hospital for some days, and fears were entertained that the laryngeal symptoms had increased, and prevented her stirring out. On the 25th, however, she presented herself, and appeared to be in good health, with a clean tongue, clear complexion, and in the possession of her natural voice. It was then ascertained that she returned home on the 14th (eleven days previously) immediately after the application of the nitrate of silver to the larynx, and remained quiet. In an hour and a half afterwards her voice returned in full power and compass, without any pain or effort, and has continued so up to this time. She took her mixture as prescribed, and at the above-mentioned date presented herself to return thanks for her cure. A laryngoscopic inspection showed disappearance of the redness of the vocal cords, the subsidence of the swelling, and restoration of the mucous membrane to its normal condition.

As she was now free from any inconvenience beyond a slight cough and a little soreness in swallowing, she was discharged cured, with a caution so to regulate the economy of her dwelling as to avoid being placed in a direct draught between windows and door.

The foregoing case is given without unnecessary detail; it sufficiently tells its own story, and shows the rapidity with which the aphonia and inflammation yielded to the means employed. It calls for no special remark beyond a reference to the strength of the solution of nitrate of silver.

ART. 112.—*A Case in which an American Copper Cent entered the Larynx, and required Tracheotomy for its Removal.*

By Mr. A. G. WALKER.

(*American Journal of Medical Science*, Jan. 1863.)

A great part of the interest of the following case consists in the fact that so large a substance as an *American copper cent* could pass the rima glottidis and enter the cavity of the larynx.

CASE.—Michel Fritz, aged 60 years, of Alleghany City, a cabinet maker, of low stature, strong frame, large muscular development, and short neck, applied on account of a distressing cough and hoarseness of four weeks' standing. The history of his case was as follows :—While playing with his child seated on his knee, he threw a *copper cent* repeatedly into his mouth, which suddenly disappeared, causing a feeling of choking, with violent coughing. Supposing the cent was resting in the throat, he ate bread and drank water, without, however, at all relieving the paroxysms of coughing; and though the cough brought on free vomiting, he became hoarse and unable to speak above a whisper. After some days, the violence of the cough subsided, as if he could not cough for want of breath. He feels as if the larynx was obstructed with something, and soreness is complained of in the part on pressure. His voice is feeble and croupy, and respiration oppressed but not accelerated. Respiratory murmur feeble, percussion normal, pulse 80. Feels chills and flashes of heat, sleeps badly, prefers the half-sitting posture in bed; is thirsty, has no appetite, and suffers from night-sweats. No mucous rales in the bronchia, nor expectoration. On passing a probang with a small piece of sponge attached into the oesophagus, no difficulty was experienced; but on withdrawing, it seemed to be arrested, behind the larynx, by some projecting body, which offered considerable obstruction to its removal. It was difficult to persuade him that the cent had found its way into the larynx; fancied that it had passed to the stomach, and would be ultimately expelled by the bowels; has been told so by his medical attendant, and the hoarseness and cough treated as a cold; but as no mitigation of his distress has been obtained by such treatment, his friends, with better judgment than his physician, insist that it must be in his windpipe, and urge him to seek admission into our hospital. I felt little difficulty in deciding, from the history of the case, from the obstruction in withdrawing the probang, and the other general symptoms, that his was a proper case for tracheotomy. Accordingly, on the 16th of March, 1856, four weeks after the occurrence of the accident, the operation was performed in presence of Doctors Hageman and Pillichody. The neck being short and plump, the integuments and deep-seated parts gorged with blood, much difficulty was anticipated during the operation. The patient being placed in a half-sitting position, with the chin well raised, an incision was made from below the cricoid cartilage to the sternum, in the median line, through the skin and cellular tissue, which were unusually loaded with fat. The edges of the incision being held apart, the sterno-hyoid and sterno-thyroid muscles were separated from their fellows by slight touches of the knife and a probe. Having arrested the profuse bleeding by cold and compression, and removed the thick layers of fat found above and beneath the deep fascia of the neck, the arteria thyroidea ima was seen running in front of the trachea, a small branch of which having been cut had to be ligatured. From the shortness

of the neck and the quantity of fat, the depth of the wound was nearly one and a half inch. The trachea being thus exposed, the tissue immediately covering it was removed by a probe, the artery carefully held aside, and the trachea was then pierced by the knife, and four of its rings divided in an upward direction. They were found rigid, from commencing ossification. Immediately on opening the trachea, violent coughing ensued, with profuse bleeding, both from the wound and from the congested mucous membrane. The patient had to be raised, the edges of the wound held apart to allow free egress to the blood from the trachea, and the breathing to become quiet. This being accomplished, the trachea was sounded by a flexible urethral sound upwards and downwards, which provoked renewed fits of coughing, with bleeding and a feeling of strangulation. Respite from coughing being again obtained, the exploration of the larynx was made with a bent polypus forceps, when the cent was detected in the larynx, the edge forwards, tightly enclosed by the swollen mucous membrane. On being seized, the forceps slipped, though moving it somewhat from its situation. At this moment the patient was well-nigh strangled. After a short pause, in a further attempt, I succeeded in grasping the cent firmly and removing it, but not without considerable force. I, however, held it preferable to bring it down and extract it by the tracheal wound, notwithstanding the force required, rather than attempt to force it upwards by the glottis, such a course being likely to lacerate the chordæ vocales, glottis, or mucous membrane, or even rupture the larynx. The patient, now finding himself relieved, was much rejoiced, and, looking at the offending body, exclaimed, that it was a costly and dearly-bought cent to him. A pledget of lint being laid in the lower angle of the wound, to favour the escape of secretions from the trachea, the edges were approximated and retained by adhesive plaster. He was placed on his side in bed, and water dressing applied; his cough continued teasing for some time, but gradually subsided; his respiration becoming more free, and his countenance calm. In the evening, his pulse becoming full, a free bleeding from the arm with strict antiphlogistic treatment was prescribed. On the following day, a further venesection was found necessary, the pulse being full and rapid; pain in the larynx, with sonorous breathing, the voice hoarse from the swollen condition of the mucous membrane; the blood was highly inflammatory. Ordered a continuance of the antiphlogistic treatment. The case from this time forward progressed favourably; the voice improved, and some expectoration followed coughing. The wound suppurated kindly, and was closed by the end of the second week, when the patient left the hospital free of cough, and his voice restored to its former healthy and natural state.

ART. 113.—*A Case in which the Shield of a Tracheotomy Tube was Extracted from the Right Bronchus.*

By Mr. SPENCE, Surgeon to the Royal Infirmary, Edinburgh.
(*Edinburgh Medical Journal*, May, 1863.)

CASE.—G. F., æt. 33, came to the hospital on June 4th, stating that, about half-an-hour before, the shield of a tracheotomy tube, which he had worn for the last twelve months, had suddenly become detached, and that the cylindrical portion had slipped down into his windpipe. He pointed to a spot corresponding to the bifurcation of the trachea, and said he felt the tube there. He spoke and breathed without difficulty; and the only signs

of irritation were severe fits of coughing recurring at frequent intervals. Mr. Spence having introduced a gun-shot probe into the trachea, guided it into the right bronchus, but failed to discover any foreign body. The left bronchus was next explored, and then the probe was distinctly felt and heard striking the metallic tube. Chloroform was now administered, and the opening into the trachea having been enlarged, a pair of long forceps, with blades bent almost at a right angle, was guided downwards into the bronchus and passed within the tube. The blades of the forceps were next forcibly expanded, and the tube thus caught was readily extracted, with immediate relief to the patient, who left the hospital quite well four days after.

ART. 114.—On Bronchocele in Domesticated Animals.

By M. BAILLARGER, Physician to the Bicêtre, Paris.

(*Medical Times and Gazette*, Oct. 4, 1862.)

M. Baillarger has recently investigated the above subject, and made several discoveries of importance in physiology and pathology. He finds that in a number of places in the departments of the Isère and Savoie, the greater number of mules had an immense hypertrophy of the thyroid body, and much larger than that generally seen in man. In one stable in Modane, amongst twenty animals, nineteen were affected with this disease; and of all the mules examined, only one-third were free from it. Amongst horses, bronchocele is not nearly so frequent, but still much more so than is generally believed. In one place seven horses were examined, which were well fed and cared for, and lodged in light and well-ventilated stables; and yet four of them had bronchocele. The same disease was found to exist, in a diminishing ratio, in dogs, cows, sheep, goats, and pigs. The goitre of these domesticated animals is no doubt due to the same endemic causes as those by which it is produced in man. The fact, that it is most frequent in mules, is in so far interesting, as these animals are sterile, and sterility is a characteristic feature of cretins. It is asserted that the drinking water of La Maurienne rapidly produces hypertrophy of the thyroid body; and that young men liable to the conscription make use of this means to escape military service.

ART. 115.—Case of Rupture of the Left Pulmonary Artery within the Pericardium.

By Mr. ALEXANDER MACKAY, Assistant Staff-Surgeon.

(*Medical Times and Gazette*, Oct. 18, 1862.)

The following very interesting case occurred towards the close of the year 1859, in a private of the 1st Bombay Fusiliers (Europeans) a regiment then stationed at Kurrachee, in Scinde, and having lately arrived down country from the Punjaub, much shattered by severe service during the late Indian mutiny :—

CASE.—Private Matthew M., 1st B. F., aged 34 years, having served thirteen years in India, a man of temperate habits, and of large muscular development, was admitted into hospital, at Kurrachee, November 18, 1859, complaining of violent attacks of spasmodic coughing, which attacks came on at intervals; was otherwise in perfect health. Surface of body cool; expectoration white and frothy; pulse 70 and equable. On auscultation the respiratory murmur was heard slightly, mixed with mucous rales; heart's sounds natural. R mist. camphoræ, ℥xij; antim. tart., gr. ij; ℥i of the mixture to be taken every third hour. Turpentine fomentations were applied to the chest until well-marked counter-irritation resulted.

19th.—Coughed frequently during the night; respiration free; expectoration copious, white, and frothy. On auscultation faint mucous rales were heard mixed with the respiratory murmur. Continue mixture as before, with fomentation.

21st.—Has had a return of the spasmodic coughing; respiration somewhat accelerated; pulse 75; heart's sounds natural; respiratory murmur normal. R mist. camphoræ, ℥viiij; tinct. opii. co. ℥ij; ℥ij three times daily. Continue counter-irritation.

22nd.—Respiration accelerated; pulse 80, and soft; heart's sounds natural; respiratory murmur clear; countenance flushed; complains of a sensation of constriction across his chest. Continue mixture as before; twelve leeches to be applied to chest.

23rd.—Respiration easy; pulse 70; chest feels much freer, and considerably relieved by the leeching. Stop fomentations; continue mixture.

24th.—Felt considerably better; respiration much more free; cough less troublesome; expectoration more copious and viscid; respiratory murmur clear. Continue mixture.

25th.—Was much improved; all cough had nearly disappeared; appetite good; pulse 70; and continued to improve generally until the 29th, when he was discharged from the hospital convalescent; to attend hospital daily.

30th.—Attended hospital this morning; had quite recovered from all cough; he, however, complained of a feeling of weight and uneasiness about his chest, but, on stethoscopic examination, nothing abnormal could be discovered. At nine o'clock the same morning, whilst seated at breakfast, he was seized suddenly with faintness, and, on being conveyed to his bed, instantly expired.

Post-mortem Examination Five Hours after Death.—On opening the thorax the lungs, heart, and pleuræ were found in their normal position. The pericardium, which, previous to being opened, appeared very tense, when opened, contained about six ounces of serum. On closer examination, the heart was found to be surrounded by a bloody clot, which, when removed, weighed about eleven ounces. The auricles and ventricles of the heart were healthy. On an incision being made along the course of the aorta, it was found covered throughout the entire of its inner surface with a thin, fatty deposit. The left pulmonary artery, on being examined, a small transverse rent was discovered on its posterior aspect to the extent of a quarter of an inch, and situated between the pericardium and the bifurcation of the trunk of the pulmonary artery. When the interior of the artery was exposed, the rent was found to be encircled by a small chronic ulcer about the size of a sixpence. The right pulmonary artery was healthy on its internal and external surfaces. The brain, lungs, liver, kidneys, and intestines were also healthy.

ART. 116.—*Ligature of an Intercostal Artery by a Peculiar Mode.*

By Dr. B. HOWARD, Assistant-Surgeon, United States Army.

(*American Medical Times*, Jan. 31, 1863.)

CASE.—J. B. Bruce, private, Co. C, 31st Ala. Vols., æt. 17, naturally a feeble youth, was wounded in the chest by a Minie ball and taken prisoner at the battle of Shiloh, April 6, 1862.

My attention was specially called to his case the second day out from Pittsburg Landing by my friend Dr. Bush, senr., of Lexington, Ky., because of the many cases of wounds of the chest on board the transport this was one of the very few in which the ball had lodged. The patient had a rather favourable appearance, and the wound looked well, with no tendency to hæmorrhage.

April 17th.—Patient was admitted to General Hospital, Louisville, Ky., with gunshot wound of the chest, fracturing the ninth rib about three inches to the left of the spinal column. The usual symptoms of gunshot wound of the lung which were present gradually disappeared. The hectic which ensued had ceased, and the patient, though very weak, was convalescing with remarkable rapidity. Medication had been discontinued, the only remaining treatment consisting in the use of simple dressing and bandage to the healing wound.

29th.—Calling accidentally at the hospital at about 1 P.M., I was informed an orderly had been dispatched to request me to visit the patient immediately. Secondary hæmorrhage had suddenly occurred, the bed was already saturated with blood, and the patient almost in a state of syncope. A medical officer had been trying in vain to stop the bleeding for about half an hour, and when I entered was controlling it by pressure on the wound. I slightly enlarged the wound, and discovering with my little finger a good many small fragments of comminuted bone, carefully removed some of them with dressing forceps. Meanwhile the arterial jet was becoming stronger and the patient momentarily weaker. There was evidently no time to be lost. Pressure at the wound was quickly supplied and steadily kept up. Stimulus was administered, and the patient being placed upon the table was put under chloroform. An incision was then made on the proximal side of the wound about an inch and a half in length, beginning about half an inch to the right of the margin of the wound, and extending along the middle of the posterior surface of the ninth rib, the middle of the incision being about an inch and a quarter from the wound in order that spicula of bone might not interfere with the subsequent steps of the operation. Having laid bare that part of the rib, and carefully defined its superior margin, I took the blunt-pointed strongly-curved needle belonging to the chain-saw, armed it with a well-waxed ligature, and introduced it, passing it immediately over the superior margin of the rib at the middle of the incision. The lips of the incised wound were well retracted, so that the eye of the needle could be sufficiently depressed to maintain the contact of its blunt point with the inner surface of the rib until it emerged at its inferior margin opposite the point of entrance. The ligature was then tied, including both the rib and artery. The bleeding stopped instantly. In order to make the success secure I repeated the operation in the same manner at the distal side of the gunshot wound. The soft parts were then brought together, and secured by sutures and adhesive straps. The patient

rallied from the effects of the chloroform, conversed rationally, but sank from exhaustion, and died about three P.M.

The autopsy relieved me of the chief objection which appeared to present itself to the operation, for after careful examination it was found that in the case of both ligatures the *pleura costalis* was not pierced.

When the usual means had failed, a tedious attempt was made, in a case that came under my observation, to secure the bleeding ends of the artery, but without success.

The operation with the needle, as described above, has the following advantages where ordinary means have failed :—

The bleeding is stopped from the first moment of seeing the patient, as it can always be controlled by pressure at the wound until the operation is completed.

Pneumothorax need not be apprehended, as the pleural cavity is not penetrated, and even though it were, the operation being subcutaneous would not be likely to cause it.

A silver wire ligature, which it would be better to use, would not produce pleuritis of a serious character, even though the pleura were pierced. The ligature could be twisted as tightly as necessary without danger of breaking, and be withdrawn at pleasure.

In the case given above, after all other means had been tried in vain, the success of the operation was instant and complete, and, but for the previous loss of blood, would probably have saved the life of the patient.

(B) CONCERNING THE CHEST AND ABDOMEN.

ART. 117.—*A Singular Case of Hernia.*

By Dr. PARKER, of New York.

(*American Medical Times*, Sept. 13, 1862.)

CASE.—A young man, enjoying vigorous health, complained for the first time of severe abdominal pain around the umbilicus and in the right groin, and some sickness of the stomach, on Monday morning. This not yielding to ordinary remedies, a physician was called, who regarded it as a case of colic, and treated it accordingly. The pain, constipation, and nausea continuing, Dr. Parker was called in consultation on Thursday. He found the patient with a rapid feeble pulse, cool surface, a somewhat tumid abdomen, with tenderness in the right hypogastrium, pain, singultus, and nausea with vomiting. On closer examination, an oblong tumour, soft and doughy to the feel, and somewhat discoloured, was found extending from the external abdominal ring upwards and outwards on the right side three and a half inches towards the anterior superior spinous process of the ileum. An examination of the scrotum showed that the testicles had never descended into their vaginal sacs. It was at once decided to cut down upon the tumour. The incision was made along the long axis of the tumour. Cutting through the integument and the common fascia, the tendon of the external oblique was divided, and the sac exposed as well as the right testis. The sac was reversed and extended towards the anterior superior spinous process. About ten inches of the ileum had escaped. It was very dark. The finger was passed in towards the internal ring, which was unusually deep, and the stricture torn with the finger. The gut brightened immediately, and was reduced without difficulty. The patient has probably done well, Dr. P. having heard nothing since the operation, three weeks ago.

ART. 118.—*Remarkable Case of Abdominal Abscess.*

By Mr. P. C. SMYLY.

(Dublin Medical Press, Feb. 4, 1863.)

CASE.—W. G., *æt.* 40, a van-driver by trade, was admitted into the Meath Hospital on the 10th of January. He complained of a swelling in the groin and of the testicle, of great pain in the abdomen, constant vomiting, and costive bowels. The bowels had not been moved for several days. The swelling in the groin he had only observed for about three weeks, gradually enlarging. On admission, he presented all the most characteristic features of intense peritonitis. He had a gonorrhœa which had suddenly stopped about or a little before the symptoms set in. His wife's account of him was that he was always drinking, and she thought he was all corruption.

The tumour in the groin distinctly consisted of two portions—the upper, about the size of a small egg, could be reduced by slight pressure, returning into the abdomen with a gurgle, and protruding again when the patient coughed, exactly like reducible inguinal hernia; the lower portion was more dense, and felt somewhat plastic under the finger. It could be pressed up the inguinal canal, but could not be passed in through the opening in the abdominal walls. The tumour in the scrotum felt more like an abscess, but the testis could be isolated from whatever caused the swelling, and on pressing this upwards a great portion of it could be pressed into the inguinal canal, making the tumour in that situation more prominent. A distinct impulse throughout the whole extent of the tumour, even into the scrotum, on coughing. There was no redness of the skin over the inguinal tumour. There was in the scrotum, the skin of which was flagged, as if an abscess was about to point.

A poultice was applied to the scrotal swelling, and the usual treatment for general peritonitis adopted. He was sinking fast when admitted into hospital.

Next morning the abscess in the scrotum was opened. There was a copious discharge of most fetid pus and a good deal of gas. When this was discharged the cavity left was again filled by thick curdy pus, which I pressed out of the inguinal canal and then through the incision. The patient seemed much relieved, vomited less during the day, and slept a little through the night. Next morning he still seemed better, the pulse fuller; however, he died in the evening.

Post-mortem examination by Mr. A. W. Foot.—General inflammation of peritoneal cavity. Small intestines congested and inter-adherent. Ascending and descending colon glued to parietes of abdomen. Omentum large; lower part full of injected blood-vessels; upper adherent in an inseparable manner to the stomach, liver, and spleen. Behind the upper part of the great omentum was a large, soft mass, which was the pancreas, enlarged and converted into an immense abscess. The contents horribly fetid, of clay-brown colour, many shreds and small portions of gland structure lying loose in it. So universal were the adhesions, and so soft and decomposed were all the adjacent parts, including the intestines (small), transverse colon, upper border of great omentum and mesentery, that it was impossible to remove any but torn fragments of the parts. The escape of brown purulent matter filled all the abdomen.

No communication between the abscess and the peritoneal cavity could be traced on account of the adhesions, but a cavity existed just inside the inguinal canal. It extended so as not to form a large tumour, the

upper part of which was exactly like a hernia. By gentle pressure this could be completely moved apparently into the abdomen, and on the patient coughing it again descended. There was no hernia whatever. The testis was perfectly healthy in structure, and not much larger than the normal size. The case was of particular interest, because in most of the books it was mentioned that abscess might be mistaken for hernia; but he had not found more than one case which bore out that view, and which is mentioned by Mr. McIlwaine.

ART. 119.—*Case in which a Breast-pin was Swallowed by a Child, and Passed by Stool twenty hours afterwards.*

By Mr. THOMAS ANNANDALE.

(*Edinburgh Medical Journal*, May, 1863.)

CASE.—One day in the month of January, 1863, a respectable tradesman in Newcastle came to my father in great consternation about his son, who had just swallowed a breast-pin. It appeared that the child, 3 years of age, had suddenly begun to cry, and the father's attention being directed to the boy's throat, he saw the sharp point of the pin sticking up from between the fauces. The father made an attempt to seize it with his fingers, but failed, and the pin passed down out of sight, and caused no further inconvenience. The accident happened immediately after the child's dinner, at twelve o'clock. The child had had for his dinner one or two mouthfuls of roast-beef, and had partaken freely of a rice-pudding.

The parents were advised to keep the patient quiet, and carefully watch his stools.

The child suffered no inconvenience, and next morning, about half-past nine o'clock (twenty hours after the accident happened), he had a motion from the bowels without any pain. In this stool the pin was found, two and a half inches in length, and with a head of the size and shape of a silver threepenny piece.

ART. 120.—*Case of Ligature of the Common Iliac Artery.*

By Mr. E. R. BICKERSTETH, of Liverpool.

(*Edinburgh Medical Journal*, July, 1862.)

CASE.—T. A., æt. 39, a strong, muscular man, by trade a boiler-maker, was admitted into the Royal Infirmary under my care on the 24th of February last. He is of sanguine temperament, of steady habits, but deaf, as nearly all boiler-makers are. Till within a few days of his admission he had worked at his trade without much inconvenience. He stated that three or four months before he had felt some uneasiness in the right side of the abdomen and in the front of the right thigh, but that until about six weeks ago there was no perceptible enlargement to be discovered. He then found a tumour a little above the right groin, which continued to increase up to the time of his admission. He could not recall any accident or other cause for its formation.

On admission, a pulsating tumour was felt occupying the entire iliac fossa, and extending in the direction of the artery from an inch and a half below Poupart's ligament to within two inches of the umbilicus. When the patient was recumbent the tumour did not cause any perceptible fullness,

except in the groin, where it was raised about an inch above the natural contour; but, on feeling through the abdominal parietes, which were very strong and muscular, the tumour was distinctly felt occupying the whole of the iliac fossa, extending laterally from the ilium to beyond the linea alba. Superiorly, owing to the thickness of the parietes, it was impossible to define its exact limits; but when the fingers were pressed deeply into the abdomen, two inches below and to the outer side of the umbilicus, the rounded upper end of the tumour was felt expanding with each pulsation, and gave the impression that the aneurismal sac extended even higher than could be felt. Pressure upon the tumour caused considerable uneasiness, without perceptibly decreasing its volume; but, from its position and shape, and from the very distinct pulsation, there could be no doubt of its being an aneurism of the external iliac artery.

For a few days the man was kept quiet in bed, directions being given that no one should examine or handle the tumour. It appeared, however, that the examination on admission had disturbed the parts; for he soon began to suffer acutely from pain down the thigh and leg, and also from pain in the tumour itself, with griping and flatulence over the abdomen generally. Opium and emollient applications afforded partial or temporary relief; but the tumour enlarged rapidly, and the abdominal pain became so severe that the patient complained greatly, and got into a restless and feverish condition. It was evident that if any operation was to be undertaken, it must not be longer delayed. From the high position of the tumour, I was convinced ligature of the external iliac would be impracticable, and that the common iliac must be tied, if, indeed, it even could be reached; for I was not without grave suspicion lest, after dividing the muscular parietes of the abdomen, I might find the sac of the aneurism extending upwards so as to involve this artery also.

The abdomen was much distended and hard from muscular contraction, depending apparently on colicky pain, from which the man now almost constantly suffered. A dose of castor oil with laudanum was ordered, to be followed the next morning by a turpentine enema should the belly continue tense.

March 4th.—The bowels having acted freely with the enema, the abdomen was fortunately much softer and less swelled. Chloroform was administered. I made a nearly vertical incision, five inches in length, midway between the anterior spine of the ilium and the umbilicus. One inch of the incision was above the level of the umbilicus, and the lower four inches passed in a slightly curved direction (the concavity being inwards) over the surface of the tumour. Having divided the skin and fascia to this extent, I dissected through the muscles and fascia transversalis at the lower end of the incision, and then introducing my finger as a guide, I separated and protected the peritoneum, and with a blunt-pointed curved bistoury cut through all the textures to the full extent of the external wound. Laying aside the knife, and using only the fingers of both hands, I then carefully detached the peritoneum from the upper edge of the iliac fossa, and from the top of the aneurismal sac, which I could feel pulsating strongly in the position in which I had hoped to find the common iliac artery. Having continued the separation of the peritoneum from off the upper end of the aneurism, and rather to its inner side, the forefinger of my right hand came upon the artery, which appeared to be somewhat displaced, being pushed inwards, and rather lifted up by the tumour, upon which it rested. My colleague, Mr. Long, greatly assisted me at this part of the operation, by pressing aside the peritoneum and abdominal contents with one hand, while with the other he introduced the narrow end of a long flat copper spatula to the

bottom of the wound and to the inner side of the artery, which was thus, with a little care and a good light, brought into view. I had now no difficulty in exposing it; and having opened the sheath with a narrow-bladed, sharp-pointed knife, I easily insinuated from within outwards an aneurism needle armed with a single silk ligature round the vessel. This was tied as tightly as possible, and all pulsation in the tumour at once ceased. No bleeding occurred during the operation, and the wound, being carefully sponged, was therefore at once closed with metallic sutures. The operation from beginning to end did not occupy more than twenty minutes. I could not satisfy myself regarding the exact spot at which the artery was tied. It appeared to be immediately beneath the umbilicus; and, as I could not feel any branch given off from it within half an inch of the spot at which I passed the ligature, I suppose it must have been very near the middle of its course, or perhaps rather nearer to the aorta. The right leg and foot were surrounded with cotton wadding and a flannel bandage, and the patient removed to bed. The subsequent progress is thus reported:—

Next day, 5th.—Has passed a more comfortable night, and has had less pain than for several weeks. The abdomen is flat and soft; the tumour smaller and harder, and free from pulsation. The epigastric artery is distinctly felt pulsating vigorously upon the surface of the tumour. The foot and leg warm, and of a healthy colour.

7th.—All going on favourably. On removing the dressings from the wound, a quantity of bloody fluid oozed from the incision by the side of the ligature, but at all other parts the wound has healed by the first intention.

8th.—The escape of bloody serum from the wound continues. There is evidently also a considerable extravasation of blood in the abdominal parietes on both sides of the incision.

9th.—Fluctuation and bulging of the abdominal walls over the crest of the ilium is felt, and arises from retention of fluid in the cavity of the wound. An incision through the skin in this situation gave vent to a large quantity of stinking decomposed blood.

10th.—Up to this date the patient has taken from one to two grains of opium night and morning, with the object of keeping the bowels quiet, and they have not acted. The man has continued quite comfortable and free from symptoms of constitutional irritation. To-day the abdomen is somewhat uneasy, the pain arising from flatulence. Ordered an enema, and to omit the opium.

11th.—The bowels have acted freely, and the patient is more comfortable. The wound looks well; but there is still a very profuse discharge of fetid matter from both incisions. Ordered six ounces of wine daily.

From this time all went on very satisfactorily: the discharge diminished daily. On the 6th of April the ligature came away, and a few days afterwards the wound had entirely healed. The man was then allowed to leave his bed and walk about. As there was some tendency to hernia, evidenced by bulging of the abdominal walls at the lower end of the cicatrix, he was directed to wear a belt to support the part. He remained in the house some time longer for the treatment of a very tight and callous stricture of the urethra, anterior to the scrotum, but was at length discharged cured on the 10th May. The aneurismal tumour had then diminished one-fourth of its original size, and still felt perfectly solid and free from fluctuation.

The most interesting pathological feature of this important case was the peculiar position of the common iliac artery, which, instead of being covered and enveloped by the sac of the aneurism, rested upon its inner

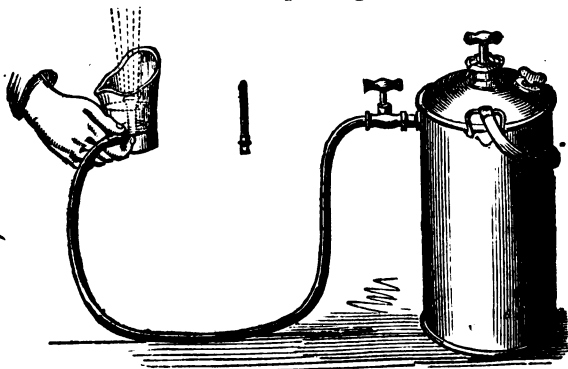
and upper surface. Had it not been for this (which analogy shows to be a most unusual disposition of parts), I believe it would have been practically impossible, owing to the high position of the aneurism, to have tied the artery without opening the tumour. The possibility of being compelled to undertake this formidable measure, or otherwise abandon the case, had occurred to me before the operation. Its feasibility and practical results in the case of subclavian and carotid aneurism, otherwise beyond the reach of surgical art, had been demonstrated in the admirable operations recorded by Professor Syme; and I was prepared to seek the artery within the tumour, if unable to reach it in the usual position. Previous experiment on the patient had proved that it was impossible to compress the aorta effectually while this was done; but still I hoped that the dissection made in a vain endeavour to find the common iliac, might at least enable an assistant to apply efficient compression on that vessel, or on the aorta, during the brief time that would be occupied in clearing out the clots and securing the artery in the interior of the tumour.

ART. 121.—*New Douche for Rectal and Uterine Disease.*

By MR. T. J. ASHTON.

(*Lancet*, Dec. 13, 1862.)

This instrument, represented in the annexed woodcut, is an adaptation of the principle of Hero's fountain, and forms the most convenient and efficient douche that has hitherto been produced. It consists of a cylinder or reservoir, about fifteen inches high and eight inches in diameter, in the centre of which is an air pump; and at the top is an opening for the introduction of water or other fluid. By a peculiar arrangement, it is impossible to pour in more than a certain quantity—about three-fourths of its capacity; so that space is reserved for the compressed atmosphere. At the side of the reservoir is a stopcock, attached to which is a flexible tube with a jet placed in the centre of an india-rubber cup or shield, which readily adapts itself to the perineal region, and effectually prevents all inconvenience from splashing.



To use the instrument, the requisite quantity of fluid is first poured into the cylinder and the plug screwed into the opening; then, by means of the pump, a sufficient quantity of air is thrown in, which, pressing on the top of the fluid, ejects it through the tube and jet with considerable force against the diseased bowel and anus, and, by the concussion and cold, constricts and gives tone to the parts. To adapt the instrument to uterine disease, the shield with the jet is unscrewed, and replaced by a female jet (figured in the cut). By means of the stopcock at the commencement of the tube, the force of the fluid can be regulated with the greatest ease.

Mr. Ashton states that he has found it very efficacious (combined with proper medical treatment) in the less severe forms of prolapsus recti and hæmorrhoidal disease, and that he has also found it very convenient as a uterine douche.

The apparatus is manufactured by Messrs. Savory and Moore, New Bond-street.

ART. 122.—*On Certain Errors in the Diagnosis and Treatment of Retention of Urine.*

By Mr. BARNARD HOLT, Senior Surgeon to the Westminster Hospital.

(*Lancet*, Feb. 21, 1863.)

After narrating five very instructive cases, Mr. Holt proceeds:—
 “The foregoing cases, which I have selected from among a considerable number that have come under my notice, will sufficiently illustrate the errors in diagnosis and treatment which are but too common. The history of each was so distinct as to make it difficult to conceive how any error could have occurred. All the patients had been in good health, and free from any urethral or kidney complications; and though in one there was slight enlargement of the prostate, in all the retention was due to paralysis of the bladder consequent on overdistension. In fact, the surgeons under whose care the cases first came were of that opinion, and attempted the introduction of catheters unsuccessfully, and then, putting the cases down as examples of ‘suppression,’ were afterwards misled by the dribbling or overflow, which they took to be the re-secretion of the kidneys stimulated by the measures they had adopted.

“The diagnosis between retention and suppression is so very easy as to render a mistake perfectly inexcusable. In retention there is the urgent desire to micturate, accompanied with violent spasms, not only of the urethra and perineum, but of the whole abdominal wall; and as time elapses the *urgency increases*, the patient rolling in agony, and straining violently to relieve himself. Besides, the surgeon’s hand will at once detect the solid tumour above the pubes, formed by the distended bladder, which will yield a dull sound on percussion. In suppression of urine, on the contrary, there is no

urgent desire to micturate, no spasm, and no agony consequent on a distended bladder; but the patient lies in a listless condition, soon passing into coma, whilst the breath and skin exhale a strong urinous smell. Moreover, the bladder will be found empty, and the fingers can be thrust into the pelvis, where the intestines yield a clear percussion sound. It must not be forgotten that a case of retention will at length pass into a typhoid condition, which might possibly be mistaken for the coma of uræmic poisoning; but the history of the case, and the presence of a distended bladder and dribbling of urine would at once point out its true nature.

"In all the cases I have seen, the error arises from the catheters not having entered the bladder. Surgeons in general practice, who are not much in the habit of passing catheters, usually introduce a gum-elastic catheter without a stilette, which, if it meets with even slight resistance, is very likely to bend upon itself, and thus never reach the bladder, although its whole length may have been introduced into the urethra. As I remarked in the early part of this paper, the injection of warm water at once clears up any doubt, and the fact that water cannot be injected may be considered conclusive evidence that the catheter has *not* reached the bladder.

"I have no hesitation in saying that in all cases such as I have described a catheter *can be passed* into the bladder, and I conceive it to be unjustifiable in any surgeon to be satisfied until he has withdrawn the urine; in which, if he will employ a metallic instrument of moderate size, he will in all probability succeed with ease. Time is of the greatest moment in these cases; and if, therefore, the surgeon in attendance do not succeed in his attempts, he is bound to call in assistance without delay, or his patient may possibly lose his life, or at least be condemned to the misery of the use of the catheter for the rest of his days.

"In Case 3 the grossest errors were committed—linseed poultices being applied to the belly and drinks given to 'force the water;' and when the second surgeon was consulted, the patient would hardly permit the catheter to be passed, because it had been so frequently attempted, and he had been assured that there was no urine in his bladder, a large quantity having dribbled away owing to the over-distension.

"When the greater part of the urine has been withdrawn by the catheter, one of two courses must be pursued: either the instrument must be introduced every four or six hours, or a gum-elastic catheter must be tied in, directions being given to the patient to empty the bladder at those intervals, with the view of keeping it nearly *empty*, so that the bladder may be able to recover its muscular tone and contractile power.

"The more I employ it, the more I feel satisfied with the use of turpentine, in ten or fifteen-minim doses, in the cases complicated by hæmorrhage from the bladder. In Cases 3 and 4 it acted at once, although both gallic acid and the muriated tincture of iron had been employed without benefit; and I think the drug deserves a more general recognition by the profession."

ART. 123.—*How to Relieve Pain in Diseased Bladder.*

By Dr. ———.

(Rev. de Thé. Med. Chir., Dec. 1, 1861; and *British Medical Journal*, Dec. 1, 1861.)¹

The presence of urine, and more especially of uric or phosphatic calculi or concretions at the *bas-fond* of a diseased bladder, sometimes produce violent pains in the bladder, and render all movement painful. In such cases, if the patient be placed on an inclined plane, which by raising the lower part of the pelvis, throws the contents of the bladder towards the upper and posterior part of the cavity (which is much less sensitive), relief is almost immediately produced, even though other means have been tried in vain.

ART. 124.—*A Case in which Two Methods of Lithotomy were carried out at a short interval.*

(Wiener Med. Wochenschr., and Schmidt's Jahrbücher, No. 8, 1863.)

CASE.—R. R., a miner, aged 41, had symptoms of lithiasis five years since. Two years ago, several small concretions were voided with the urine. Examination with the catheter proved the existence of a hard, smooth body in the left side of the bladder. After performing the lateral operation, it was found impossible to extract the same, it being fixed in and firmly surrounded by a portion of the bladder which could not be reached. The patient made a rapid recovery, with the stone remaining in its place. Only a small part of it had been crushed with great exertion. A very annoying enuresis followed in a short time, with increase of the other troubles. Five weeks after the first operation, the high operation was executed. With some difficulty the concretion could now be removed; it formed a stone of an oblong shape, two inches nine lines long, and nearly two inches broad, weighing a little over three ounces, and consisting principally of uric acid. Some inflammation followed, but in eight days all the organs had resumed their normal functions; on the ninth day, the urine commenced to pass through the urethra; after the fifteenth not a drop of it appeared in the wound. Recovery, however, was slow this time, and it took sixty-four days to heal the external wound. Finally, however, the case terminated in complete recovery.

ART. 125.—*A New Method of Lithotomy in the Female.*

By Dr. ROBERT NELSON.

(American Medical Monthly, Sept. 1862.)

Dr. Nelson cuts into the bladder from the vagina, making his incision beyond the sphincter of the bladder. His object is to avoid the common risk of losing the power of retaining the urine, which risk is incurred where the sphincter is divided.

CASE.—Mrs. G. W. R., thirty years of age, mother of two children, the

last four years old, has suffered slightly for ten years, at times, in urinating. Four years ago she came under the present fashionable treatment for uterine disease—such as leeching, scarifying, causticating, &c., of the os uteri, without relief. Since last December her suffering increased, and prevented her from going about, and in January she had to keep her bed nearly all the time, to avoid the severe paroxysms that exercise would induce. In the early days of August it was discovered that her sufferings were due to a calculus in the bladder. She then came under my care. It was evident that the only remedy that could relieve her was the removal of the stone. The question to be considered was, what method would answer best. Dilatation of the canal and lithotripsy had previously disappointed me; besides that, the state of her bladder was such—exquisitely sensitive, ulcerated, and the urine passed was thick with ropy mucus and pus—that crushing of the stone into fragments, many of which would remain behind, and so increase the actual disease of the bladder as to render such an operation dangerous, that lithotripsy could not be thought of. There remained but lithotomy to be tried; but, looking back to the risk of losing the power of retaining the urine should the sphincter be divided as in the usual operation, I decided to avoid this serious accident, by not interfering with the sphincter, but to enter the bladder through the vagina, beyond the sphincter. Accordingly, on the 6th of August, having administered chloroform, the bladder slightly injected with water, and a staff introduced through the urethra, the knife was thrust through the roof of the vagina in the mesial line, and carried about an inch and a half towards the uterus, making an opening of sufficient size in the dilatable part of the bladder to admit the common lithotomy forceps, which were readily introduced, and the stone at once seized and slowly drawn out. The stone being very porous and friable, a portion of it was crushed in the forceps, and had to be clawed out, and the finer particles washed away by injections of tepid water. The next step was to close the lips of the wound neatly and exactly, and thus to retain them. This was effected by five sutures of common flax thread, as more simple, fitting better, and quite as effective as the wire material. A No. 12 female catheter, with two large fenestræ, armed with a shield, to prevent its entering too far into the bladder, was introduced, and retained *in situ* by an elastic cord. The stone was $1\frac{1}{2}$ inch long and $1\frac{1}{2}$ thick, studded with very rough eminences; it was very porous, and of the phosphatic kind.

All her previous distress vanished, of course; passed the remainder of the day well: slept well that night, and the same for the subsequent time. The catheter becoming plugged with tenacious mucus, had to be withdrawn every two or three hours for a couple of days, to clear it. The bladder then recovered rapidly, and the mucus diminished. On the sixth day the sutures were cut and removed, the wound being quite healed by the "first intention." On the seventh day she got up to her meals with the rest of the family, and on the tenth day my attendance ceased. From the first to the last, not a drop of urine escaped through the wound into the vagina. She has continued perfectly well ever since, going about visiting her relatives, in a state of comfort and happiness unknown to her for a long time past.

I am aware of the saying that "one swallow" does not make summer, and that there are those who will say that this was a "lucky case;" and that it will often be followed by a vesico-vaginal fistula: true—in the hands of even well-informed surgeons, but who do not possess that mechanical ability which is indispensable to a good and successful one. However, I am satisfied that in the hands of the latter there can be no excuse why every case should not turn out as well as this one.

ART. 126.—*Case in which a Fragment of a Broken Gutta-percha Bougie was Extracted from the Bladder through the Urethra.*

By Mr. CHRISTOPHER HEATH, Assistant-Surgeon to the Westminster Hospital.

(*Lancet*, Dec. 6, 1862.)

CASE.—R. S——, aged 49, applied to Mr. Heath, among the out-patients, on the 21st of October last, to be relieved of a piece of gutta-percha bougie which he said he had broken into his bladder. He had suffered from stricture of the urethra for fifteen years, and had been in the habit of passing bougies for himself. Latterly, however, his No. 6 elastic bougie had become too large, and he made himself one of gutta-percha tapering to the end. On the 27th of August last, he was using this, and got it into the stricture, when it broke off, leaving only three inches in his hand. He managed to extract a piece three inches long with his fingers, and then applied to the house-surgeon of the Homœopathic Hospital to be relieved from a distended bladder. The house-surgeon of that establishment pushed the bougie into the bladder with a catheter, and drew off the urine; and, on the following day, tried to wash the piece out with a double catheter. He afterwards went into the Homœopathic Hospital, but only had a catheter passed occasionally, with one exception, when, according to his account, Mr. Holt's dilator was used, but only with a small tube, and therefore did no permanent good. Mr. Heath examined him, and found that his stricture would only admit a No. 5, and was unable to detect the bougie with a small sound. He was admitted into the hospital under the care of Mr. Heath, on Oct. 28th, and next day Mr. Heath introduced Holt's dilator, and split the stricture up to No. 11 size, drawing the urine off with a large-eyed catheter, so as to favour the discharge of the piece of bougie, but nothing passed. The next day the catheter was again introduced without any result. Careful sounding failed to detect the bougie. The patient had some shivering in the evening, and passed some blood; and it was uncertain whether the bougie had not come away with it.

Nov. 4th.—The bladder was carefully sounded by Mr. Heath, but nothing was detected, nor were any of his colleagues who tried more successful.

7th.—The patient having felt the bougie again, Mr. Heath examined the bladder, and, having emptied it, felt the bougie with the sound. He therefore introduced a lithotrite scoop, and after considerable trouble caught and extracted a piece of gutta-percha bougie, three inches and a half in length, tapering from about No. 5 to No. 1 catheter. The bougie was much curved upon itself, and somewhat corroded; and there was a slight phosphatic deposit upon it.

10th.—Has had no further irritation in the bladder; but feels very sore in the urethra, particularly if he has an erection.

18th.—No. 11 catheter has been passed with ease two or three times. Made an out-patient in order that an instrument may be occasionally passed to maintain the size of the urethra.

ART. 127.—*Case of a Female Child in which the Urinary Bladder was Inverted through the Urethra, with large Prolapsus of the Rectum.*

By Dr. BEATTY.

(*Dublin Quarterly Journal of Medical Science*, August, 1862.)

CASE.—A female child, aged one year and eleven months, was sent from the country to the City of Dublin Hospital, with a statement that there was something wrong with the genital and urinary organs. She was a fine, strong, handsome child. The appearance of the parts was most extraordinary. Just between the labia there was a scarlet tumour about the size of a chestnut, formed by the inner surface of the bladder, and similar to what has been seen in cases of vesico-vaginal fistula, or of malformation where the anterior wall of the abdomen is open above the pubes, and the inner coat of the bladder protrudes.

Upon touching this tumour with the finger the child cried violently: by pressure it could be forced back and even replaced; and the urethra was sufficiently large to admit of the easy passage of the forefinger into the replaced bladder, showing the case to be one of complete inversion of the bladder through the urethra. The mother told Dr. Beatty that the inversion of the bladder did not take place till the child had a fit of crying when it was twelve months old. There was also a large prolapsus of the rectum, which occurred when she was nine months old, in consequence of an attack of diarrhoea. The child remained in hospital up to Monday last, and continued quite well. Dr. Beatty kept her in for the purpose of devising some means to remedy the defect. On Sunday morning, however, she was attacked by severe croup, which terminated in her death on Monday evening. He was thus enabled to obtain the specimen now before the society, showing the bladder turned inside out through the urethra. It had now lost some of the scarlet colour, and something of its size. In the prolapsus of the rectum there was nothing remarkable; but, combined with the other defect, it gave the parts a very curious appearance. The uterus was *in situ*, and the ovaries were very large for a child of her age. Dr. Beatty had never seen another instance of such inversion. The child died of croup.

(c) CONCERNING THE UPPER EXTREMITIES.

ART. 128.—*On the Expediency of Opening the Capsule in Hip-joint Disease, with a view to Cutting Short the Painful Phase of this Disease.*

By Mr. BARWELL, Assistant-Surgeon to the Charing-Cross Hospital.

(*Lancet*, Jan. 10, 1863.)

In some excellent lectures on hip-joint disease, Mr. Barwell says:—

“We have seen that a violent exacerbation of the disease accompanies the great distension of the capsule just before its rupture, which in its turn is followed by remission of all the distressing

symptoms. Now, there can be nothing more reasonable in idea than to endeavour to cut short the painful phase by artificially relieving the distension of the capsule. Dr. Bauer, of Brooklyn, in America, is, I believe, the first surgeon who proposed to open a joint subcutaneously, with the intention of relieving its inflammatory condition, and without reference to its contents. I can bear decided witness to the value of such practice whenever the capsule is distended; and in such cases the operation is not difficult, but the patient should be brought under the influence of chloroform. A flat and slightly curved trocar and canula is the best instrument for the purpose. It is to be inserted about three-quarters of an inch behind the upper end of the trochanter, and passed onwards and a little forwards. The hand holding the instrument will detect immediately when it has entered the cavity. It may then be pressed a little more home, and the trocar withdrawn. The thigh is next to be inverted and rotated inwards. Pressure with the hand may be made both on the groin and behind the trochanter, which manœuvres will as far as possible procure the emptying of the capsule. While withdrawing the canula, the finger must be placed over its outer opening; the wound must be closed at once, and the limb placed in the splint. By this simple procedure we spare the patient a great deal of very severe and wearing pain, evading that period of intense suffering which precedes the shortening. We lessen the widely destructive effects of the inflammation, and we render the subsequent course of the disease more manageable. Moreover, if the fluid in the capsule be pus, we certainly do to a very considerable extent diminish the probability of subsequent abscesses, which are so generally produced in the neighbourhood of hip-joint disease. Indeed this method, combined with other and more essential treatment, will prevent such occurrence altogether."

ART. 129.—*Reunion of Intra-capsular Fractures of the Femur.*

By M. FABRI, of Bologna.

(*Presse Méd. Belg.*; and *London Medical Review*, Aug. 1862.)

M. Fabri, of Bologna, has published some interesting cases in which reunion followed this ordinarily intractable fracture. The first was that of a man whose body was brought to the dissecting-room, and who had long used crutches. An intra-capsular fracture was discovered which had been perfectly reunited by the formation of an osseous substance. Previous to the occurrence of osseous union, however, so much displacement and separation of the fragments had taken place that lameness resulted. The spongy substance of the head was united with that of the neck by the medium of a compact layer of new bone, four millimetres thick. In the second case (a man æt. 70), the circumstances were similar, only the union was by means of singularly dense cartilage, instead of bone. In neither of

these cases was there any penetration of the neck of the femur into the spongy tissue of the great trochanter. In a third case (a man *æt.* 77) the patient was able to walk in less than a year after the accident, which he survived for seven years. Complete osseous reunion was found, on *post-mortem* examination, to have taken place.

ART. 130.—*A Case of White Fibro-serous Discharge from the Thigh.*

By Dr. A. B. BUCHANAN, Physician to the Dispensary for Diseases of the Skin at Glasgow.

(*Proceedings of the Royal Medico-Chirurgical Society, Jan. 27, 1863.*)

The paper containing the account of this case was illustrated by specimens of the milky discharge and by a drawing of the diseased surface. The patient, it appears, was under treatment by Dr. Buchanan at the time the paper was written.

CASE.—The patient was a woman, in other respects in fair health, aged forty-six, and mother of six healthy children. The discharge was white, like milk, and flowed from excoriations produced by the rupture of small vesicles scattered over the back of the thigh; and particularly from an infiltrated patch, of the size of the palm of the hand, on which the vesicles and excoriations were most abundant. The milky fluid coagulated a few minutes after being passed. It contained a fatty molecular base similar to that of chyle, and a few nucleated cells. The results of a chemical analysis closely corresponded with those yielded by "chylous urine."

The patient dated the commencement of her malady from a shivering fit twenty-one years ago, shortly after which she noticed a "lump" in the situation of the affected surface. Fifteen years ago vesicles appeared, from which a brownish fluid exuded on scratching. For the last six years the discharge has been milk-white, and is always worst in wet weather, and while the patient is walking about, when its amount may be half a pint per hour. At night it ceases to run, recommencing about an hour after the patient rises in the morning; but in dry or frosty weather it may occasionally be absent for a week or two. The veins of the affected limb are varicose, but no enlargement of the lymphatic glands could be detected. The author succeeded in controlling the discharge for two months by a long elastic stocking; the use of which, however, had to be discontinued, owing to severe lancinating pains in the thigh. The breasts also swelled, and began to secrete small quantities of cholestrum. Immediately on ceasing to use the stocking the uneasy sensations subsided, and the discharge commenced anew.

After remarking that cases of this affection are extremely rare in temperate latitudes, Dr. Buchanan refers to several recent examples which prove that they were probably more frequent in warm climates. He cites, however, two unequivocal cases of the same affection, both dating from the seventeenth century; one in a male subject occurring in Germany, and the other, in a female, in France.

Referring to various pathological theories, the author fully recognises the identity of the above disease with "chylous urine," or as

he prefers to call it, "white fibro-serous urine." He objects particularly to the theory which identifies white fibro-serous discharges in general with chyle. He gives his reasons for believing that it is more natural to consider them as equivalent to the white liquor sanguinis—to transudations of the serum of the blood during its periodical milkiness after meals, but with certain modifications inseparable from the mode of its secretion. Thus, while the water, albumen, and salts, and possibly also the fibrin, would come from the blood directly, there is reason to believe that the cells present in the discharge must be derived from the secretory layer of the skin, or from the epithelium of cutaneous glands. He contends that the molecular base is unquestionably derived from the blood; but that the molecules could not be conceived to filter directly through the walls of the vessels without presupposing the existence of a uniformly and intensely milky serum while the discharge was flowing, even at long intervals after meals. To avoid this difficulty, it might be supposed that the epithelial cells of the glands of the skin had the power of separating, by a perverted function, fatty matter from the blood, much as the epithelial cells of the intestine are concerned in filtering it into the lacteals. The cells would then become gorged with fatty molecules, and the uniformly white colour of the discharge would be accounted for without its being necessary to suppose that the liquor sanguinis was ever milky except, as usual, after meals. On this view, white fibro-serous discharges would depend immediately on deranged glandular action, and the foregoing case might be defined as a rare functional affection of the glandular apparatus of the skin.

ART. 131.—*Removal of the whole Shaft of the Femur.*

By Mr. J. G. BEANEY, Surgeon to the Melbourne Hospital.

(*Medical Record of Australia*, Dec. 24, 1862.)

CASE.—The patient, a male, aged 21, twelve years in the colony; he has always had good health, and his family are healthy. In the month of October, 1860, he walked a short distance after dinner on a wet day, but did not get wet. He sat down for a short time in a room, when, feeling cold, he went to the fire. He then felt pain in the leg near the knee, and walked lame. The lower part of the thigh began to swell, and the swelling gradually extended up the thigh; he had fever, with symptoms of inflammation of the bowels, and sweated profusely. At the end of six weeks, matter formed near the upper part of the thigh, and two or three weeks later a large quantity escaped. Other collections of matter ensued, and broke. The openings formed have continued to discharge. The thigh-bone is very much enlarged, but the enlargement is most marked near the upper third; here he suffers from pain of a dull, aching character; the pain has been much severer than it is at present, relief following the escape of some thick yellow matter. The leg is twice the size of the opposite one, the skin, cellular tissue, muscles, and bone being united together. There are seven or eight openings—three or four very large ones—some discharging whey-like fluid, others thick pus. He has not had night-sweats since the leg

began to discharge. He has gained flesh during the last twelve months; his appetite is good, tongue clean, pulse 78, and lungs healthy. No diseased bone could be felt through any of the sinuses.

Amputation of the thigh had been advised by several surgeons, but feeling convinced that the case was one of acute inflammation of the shaft of the femur, followed by its death and the formation of a casing of new bone, I therefore determined to cut through the new bone and remove it. On the 24th of January he was placed under the influence of chloroform; a semi-circular incision was then made in the centre of the outside of the thigh, and the flap thus made dissected from the bone. By means of a strong gouge and mallet the new bone, which was three-fourths of an inch thick, was cut through, when about a fourth of a pint of watery pus escaped. The whole of the shaft of the femur was found necrosed. In some parts it was not thicker than a quill, in others as thick as the thumb. It was so closely embraced by the new bone; that the greatest difficulty was experienced in extracting it. Scarcely a table-spoonful of blood was lost. The wound was filled with wet lint, to prevent its closing. He had no bad symptoms. Several pieces of bone escaped from the wound, which was kept open with considerable difficulty. One month after operation, the limb was very little larger than the opposite one, and the tissues felt soft; pulse, 86; appetite good. He has gained flesh. There is a prominence on the lower part of the shaft of the femur, and a small opening exists opposite the outer condyle. When a probe was introduced, roughened bone could be felt. Early in April I cut down on the outer side of the condyle, and removed two large thin pieces of the diseased external condyle. A sinus was then discovered passing through the new bone to the centre of the enlargement of the lower part of the femur, and loose bone could be felt with the probe. The new bone was cut through with the gouge, and a piece of necrosed bone, the size of a large walnut, removed. The capsule of the knee-joint was not touched; the length of time the knee had remained motionless had caused slight anchylosis of the joint, and the capsule to diminish in size. From this time he progressed most satisfactorily. For some time there was a free discharge of laudable pus, accompanied occasionally by small spiculae of bone. The latter, however, ceased to come away in September, and the wounds rapidly closed. In October, he left for the country with a good limb; motion was being gradually restored to the knee-joint—the opening made on the outer aspect of the thigh would scarcely admit a probe, and the whole limb was not larger than its fellow. He was in sound health, and gaining flesh rapidly.

ART. 132.—*On the Treatment of Varicose Veins of the Leg by Ligature and Division of the Vein above the Ligature.*

By Dr. ———

(*Medical Record of Australia*, Dec. 24, 1862.)

The following is the plan employed:—The vein having been rendered prominent, either by allowing the leg to hang down or by tying a piece of tape round it, a needle is passed under it. The leg is then raised and the vein emptied by pressing the blood up with the finger beyond it. A piece of soft india-rubber, or wax bougie, or jeweller's wool is placed over the vein, and the twisted suture applied, tight enough to bring the walls of the vein in close

contact, but not to produce pain. The vein is then divided about a third or half an inch above the ligature, either subcutaneously or with the skin. The ends of the needle having been cut short, a pledget of wet lint is applied, and a bandage from the foot up, tight below the point where the needle has been introduced, but rather looser over it. The leg is then placed on a pillow, the patient being strictly confined to bed for two or three days. The blood coagulates in the vein below the ligature very quickly, and the wound in the vein soon heals. As soon as the former is firm enough and the latter united, so that there is no risk of hæmorrhage, the needle may be removed, and this in from 24 to 60 hours, but it generally causes so little inconvenience that it may be left for several days. It is a good plan to divide the vein immediately below a valve. In cases in which ulcers exist with great induration of the subcutaneous cellular tissue, the vein should be tied and divided above it. The abstracts of four cases are given in illustration.

(D) CONCERNING THE INFERIOR EXTREMITY.

ART. 133.—*Dislocation of the Left Shoulder reduced by Manipulation, without the Employment of Anæsthetics or other Remedies.*

By Dr. CHARLES H. PILE, Assist. Surg. U.S. Navy.

(*American Journal of Medical Science*, Jan. 1863.)

CASE.—On the morning of October 9th, I was called to see a sailor suffering from an injury of the left shoulder, produced by a fall on deck. On examining the injured part, I discovered a luxation of the humerus forward, the head of the bone forming a prominent tumour under the belly of the pectoralis major muscle; the acromion process of the scapula was prominent and well defined. I immediately proceeded to reduction. I seated the patient on a low stool, flexed the forearm on the arm, elevated the arm at an angle of 45° with the body, then rotating the head of the humerus by turning the arm backwards as far as possible, and afterwards suddenly reversing the motion on carrying the injured extremity across the chest towards the sound side, when the head of the bone slipped into the glenoid cavity with a slight noise. This process for reducing dislocations of the shoulder was taught me by my old friend and preceptor, Prof. H. H. Smith, of Philadelphia.

The advantage it possesses over the old method is very manifest, since, instead of requiring a vast expense of muscular power on the part of the surgeon, it is nearly all transferred to the muscles of the patient. In flexing the forearm on the arm, the flexor muscles are relaxed; by elevating and rotating the head of the humerus, it is dislodged from the neck of the scapula, and gradually forced upon the edge of the glenoid cavity, when the supra-spinatus, deltoid, and infra-spinatus muscles quickly draw it into its proper place.

ART. 134.—*Gangrene of the Arm consequent upon Tattooing.*

By M. NINON.

(Méd. de Gironde, Mai, 1862 ; and Schmidt's Jahrbücher, No. 7, 1862.)

The practice of tattooing is widely spread through the French army and marine, and many serious accidents have been known to result from it. These have generally been ascribed to the colouring matters (often containing mercury) which are introduced beneath the skin; but it is quite as likely that the kind of instrument used (*viz.*, an elder-stick in which from three to five needles are set almost close together) is the cause of mischief, seeing that the needles often get rusty, and doubtless retain a good deal of organic matter after each operation, and also that they inflict considerable damage on the cellular tissue.

CASE.—The present case is that of a young marine, who, being in hospital for the cure of an acute rheumatism of the shoulder, allowed himself to be repeatedly tattooed. The particular tattooing which caused such mischief was executed just below the deltoid muscle on the arm, and was a very elaborate representation of Freedom, armed and holding the tricolor flag in one hand, and a sword in the other. This artistic decoration so irritated the skin that gangrene of the upper arm set in, and necessitated amputation at the shoulder-joint. Fortunately the man recovered.

ART. 135.—*Injury of the Arm common to Children of from One to Four Years of Age.*

By Dr. R. M. HODGES.

(Boston Medical and Surgical Journal, Sept. 18, 1862.)

Dr. Hodges relates three cases of an accident often observed in children, and not unfrequently puzzling the practitioner, partly in consequence of the impossibility of persuading the little patient to submit to a careful examination, and partly in consequence of the suddenness with which the symptoms often disappear.

CASE I.—Feb. 2, 1862. A nurse, holding the hand of a little girl twenty-eight months old, in a moment of impatience gave it a sudden twitch. The child immediately cried out, would not allow herself to be touched, and held her arm motionless. Her mother, however, placed it in a sling, and three hours after the accident the limb was found in the following condition, *viz.* bent at an obtuse angle, and resting against the body, the forearm much pronated. There was no apparent swelling or deformity, but the slightest motion was extremely painful, and the little patient was unable or unwilling to hold anything in her hand. On manipulating the limb there seemed to be some impediment in its natural movements, and in attempts to produce certain of these a feeling suggesting crepitus was detected; but before either the seat of this, or the actual diminution of mobility could be determined by examination, the abnormal condition of things vanished, the child ceased to cry, took her playthings in her hand, and the limb was evidently once more in its natural condition.

CASE II.—Feb. 7, 1862. A little girl, thirty months old, fell whilst walking across the parlour floor, her mother holding her by the hand. When seen, her limb presented a condition precisely analogous to that described in the preceding case—there was the same pronated and semi-flexed position; the child was unwilling to use it, and cried the moment it was touched. Before I could ascertain what the exact injury was, manipulation had produced the obscure crepitus, or something which seemed like it, the pain and immobility instantly disappeared, the child readily took its playthings, and with the exception of a little fright, was herself again.

“These two cases occurred in the course of the same week, and were the first and only ones of the kind which had ever fallen under my observation. I had considered them both as instances of incomplete dislocation of the head of the radius forward. This accident, or, at all events, this phase of it, is not described in the common works on surgery, although a thesis in French, one or two articles in journals, and a description by two or three French writers may be found, in all of which it has been looked upon as some form of dislocation of the upper extremity of the radius. M. Goyrand, of Aix, however, who has written several articles on the subject, takes an entirely different view of the matter. In a paper recently presented to the Surgical Society of Paris (*L'Union Médicale*, Nov. 23, 1861), and which has fallen under my notice since the occurrence of the two cases above described, he sums up the symptoms of this accident so accurately that I venture to quote his description, believing that, in the absence of any readily accessible account of the injury, its recapitulation, with an abstract of some of his remarks, will not be altogether amiss.

“A child, from one to three or four years of age, is saved from a threatened fall by being suddenly caught by the wrist, or it is lifted over a gutter, or made to step up by a violent pull of the pronated hand. At the moment of the strain, the person holding the hand notices a slight snap or shock. The child cries out and continues to complain; the limb hangs motionless by the side, a little in front, with the elbow partly bent and the hand pronated. Any attempt to supinate the hand increases the cries, the rotation outwards is brought up by a mechanical obstacle, and if this is not overcome, as soon as the hand is let go it returns to its pronated position. This symptom is pathognomonic. If the resistance is overcome, a snap is heard or a slight jar is felt, pain ceases, motion is restored, and no trace of the accident remains. If nothing is done by the surgeon, after a certain period, which varies from a few hours to several days, the limb spontaneously resumes its natural condition, ordinarily during sleep. If any length of time elapses prior to the reduction, either by the surgeon or by the efforts of nature, the hand becomes abducted, and there appears on the dorsal surface of the wrist and the neighbouring parts of the forearm a painful swelling similar to that accompanying fractures, dislocations, or sprains.

“M. Goyrand had always considered the lesion in these cases as a partial dislocation of the radius at the elbow, but so slight as to give no appreciable deformity of the articulation. In 1857, a case

occurred which revealed to him what he now conceives to be the true nature of the injury, viz., a displacement of the inter-articular fibro-cartilage of the wrist in front of the carpal extremity of the ulna. In complete pronation, the inferior articulating surface of the head of the ulna is exposed behind the corresponding border of the fibro-cartilage by more than three-quarters the thickness of the bone; and hence, in a forced rotation of the radius in the direction of pronation, it is conceivable that the fibro-cartilage may easily be carried in front of the head of the ulna. This displacement occurs only in childhood, as it can only be produced by great violence; in adults, forced pronation is prevented by antagonistic muscles brought into action as soon as pronation becomes painful. This cannot be done by a child. The varying size of the head of the ulna explains why some children, exposed to a cause capable of producing this displacement of the fibro-cartilage, escape the accident.

"The following cases are submitted in justification of this diagnosis, and as illustrating the symptoms developed by the accident:—

CASE a.—Mary J., one year old, just beginning to walk. July 6, 1861, whilst walking, being held by the right hand, her brother, seven or eight years old, fell heavily against her. The nurse, squeezing its hand, pulled the child quickly towards her, and in doing so felt something "give way" in the little arm. The child immediately began to cry; the limb was held motionless, resting against the body, carried a little in front, and with the forearm pronated. No deformity about the wrist or elbow. Motion or pressure about the elbow causes no pain, but pressure on the back of the wrist makes the child cry out. On supinating the hand, the movement was felt to bring up against some resistance, the child cried louder, and the hand when released assumed its previous pronated position.

The arm being held by the mother, M. Goyrand seized the hand in his right, with the thumb on the lower end of the radius and the forefinger on the head of the ulna; with his left hand he gently supinated that of the child. Before supination was completely effected, the finger resting on the end of the ulna felt a slight shock; instantly the resistance to supination was overcome and reduction accomplished.

Two minutes afterwards the child took in its injured hand a bracelet held out to it by its mother. The mobility was completely regained, and all uneasiness and pain gone.

CASE b.—Margaret D., thirty-five months old, held by the left hand, made a false step, and from the sudden jerk which the hand in a state of pronation received, there resulted the injury which M. Goyrand makes the subject of his paper. One of his pupils, a physician in the town where the child's parents reside, being called to the case immediately on its occurrence, recognised the injury and supposed he had reduced it. The symptoms of the displacement did not, however, disappear, for on the following day the limb was immovable and in a pronated position. M. Goyrand saw the child fifty hours after the accident, and noted the following symptoms, viz., those which are mentioned in the previous observation, and in addition, on the dorsal surface of the wrist, a swelling extending to the neighbouring parts of the forearm and hand; this was extremely tender to the touch, especially at a point corresponding to the articular interspace of the wrist. There was a slight inclination of the hand to the ulnar side of the limb. In reducing the displacement, the finger which rested on the carpal extremity of the ulna felt, distinctly, when the hand was brought into supi-

nation, a crepitus precisely like that in a case of fracture. Before supination was completely produced the sensation ceased, opposition to supination was overcome, and the reduction was accomplished. Thirty-six hours afterwards no trace of the accident remained.

In both these children the displacement happened again in the same arm. In other cases which have been noticed, the two limbs have been successively the subject of the accident, one after the other.

In the discussion which ensued upon the presentation of this paper, it appears that MM. Guersent, Marjolin, Giralles, and Velpeau were at a loss to accept M. Goyrand's explanation. M. Guersent believed that the cases in question comprised a variety of lesions with certain identical symptoms. M. Marjolin thought that in many of these cases he had seen a deformity about the elbow. MM. Giralles and Velpeau thought that a dislocation of the fibro-cartilage could not take place without a lesion of the serous membrane of the articular cavity, after which consecutive symptoms should be developed, no matter how simple the reduction might be. Are we then to accept this explanation of the phenomena presented by this injury? Dr. Hodges answers:—

“Although M. Goyrand fortifies his opinion by the results of experiments on the dead subject, he admits that the fibro-cartilage will not remain displaced unless the hand is held up and in forced supination. In two attempts which I have made upon very young subjects, I have not been able to produce this dislocation, nor when the joint was opened have I been able to force the fibro-cartilage into the position which M. Goyrand assumes that it takes, without tearing it from its attachments. Any one who will examine for himself the very limited range of displacement of which this cartilage is capable, except by the use of violence which must determine more marked symptoms than ever follow this accident, will be convinced of the error of M. Goyrand's views. Although no two writers agree upon the precise nature of the injury, all of them, with the exception of M. Goyrand, locate it at the elbow. This diversity of opinion is accounted for by the fact, that before its seat can well be ascertained the symptoms disappear with the very first exploratory manipulations. It would seem, from the weight of testimony and from my own experience, that without attempting to account for this somewhat peculiar, and, as I judge, not unfrequent accident, by any ingenious theory or complicated displacement, a satisfactory explanation of its pathology is to be found in the partial luxation of the head of the radius, which, either in one direction or another, is accepted as its rationale by Duverney, Bouley, Monteggia, Martin, and Fougau d'Etampes. This view finds support in the position assumed by the arm, and the manner in which the symptoms of the injury disappear. Rotation and flexion (which of course, if carried through the whole range, implies extension) are the first motions which every surgeon gives to an arm when he examines it for a suspected fracture or dislocation; and in a child, where this bone was but partially out of place, might well be adequate to the reduction, even when practised gently and but a single time. An adult would perhaps unwittingly or of his own

accord reduce a dislocation of this slight degree, when in a child, from pain and fear, it might remain persistent till surgical aid arrives. In the absence of this, we are told that in the course of time spontaneous cure is effected. Would this occur if there were so serious a displacement as that of the interarticular fibro-cartilage of the wrist? It appears then that the following opinions present themselves :—

“1st. That of M. Guersent, who conceives that the cases in question comprise a variety of lesions with certain identical symptoms. 2nd. That of a number of writers, who attribute the symptoms to a partial dislocation of the head of the radius in one direction or another. 3rd. That of M. Goyrand, which has been detailed at length.

“The cases present too striking a similarity to admit of the acceptance of the first of these, and of the third we have already given our opinion.

“The second, therefore, seems to merit the most favourable verdict, and the latitude which it permits is, in the present state of our knowledge, no more, perhaps, than it is discretionary to allow.

“Since writing the above, a third case has fallen under my notice.

CASE III.—A child ten months old was brought to the dispensary, June 24th, 1862, with her hand in a sling, having received an injury to the elbow the afternoon previous, by being lifted up with a jerk by the wrists whilst lying on its back, in which position its grandmother had been putting on a diaper. The symptoms of injury—to wit, crying and inability to use the arm—ensued at once. On examination the following morning, the characteristic position, semiflexion and pronation, and pain on motion, with the absence of deformity or swelling, led me to suspect the nature of the accident, and I was able to satisfy myself that the seat of injury was not at the wrist. The opposition to rotation was evidently at the elbow, and a single sensation of crepitus was felt at this point; but on manipulating to detect, if possible, the condition of the parts, the resistance to supination was unexpectedly overcome and the normal state of things assumed, before I had ascertained what the precise derangement was which stood in the way of free motion. The experience of this case confirms me in my opinion of the erroneousness of M. Goyrand's views, at least in their general application.”

PART III.—MIDWIFERY.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

(A) CONCERNING PREGNANCY AND PARTURITION.

ART. 136.—*A Case of Labour at the Eighth Month, and Delivery of a Living Child in less than Four Hours, by Dr. Barnes's Method.*

By Dr. W. B. WOODMAN.

(*Lancet*, Jan. 3, 1863.)

Dr. Woodman is acting resident accoucheur at the London Hospital. The case itself was under the care of Dr. Barnes.

This case, and others related in Dr. Barnes's *Memoirs* (Abstract XXXVI. p. 296) must dispel all doubts—we have heard such expressed—as to the perfection of the new method of inducing premature labour at a predetermined hour.

We observe that in November last M. Tarnier read to the Académie de Médecine of Paris a memoir, describing a method of inducing labour by the use of uterine dilators. From the reports in the French journals it does not appear that any reference is made to Dr. Barnes's prior publications. Although there is every reason to believe that the idea of M. Tarnier's instrument is borrowed from the English obstetrician, he does not appear to have equalled him in the success of his operation. In October last, we are informed, M. Blot, while doing duty at the Clinique d'Accouchements for M. Dubois, using Tarnier's instrument, had not succeeded in completing delivery after two or three days' proceedings.

CASE.—Eliza J—, aged 30, attended as an out-patient, on account of incessant vomiting, which had persisted from the commencement of her pregnancy till now—the eighth month. The moment food is taken it is rejected. She has rapidly lost flesh, and is now much reduced in strength. She gets no sleep at night from pains in the loins, inside of the thighs, &c. The usual remedies, including the oxalate of cerium, have been tried without avail. She has been married eleven years, and was only pregnant once before—ten years ago. She suffered then from obstinate vomiting, from which she

was relieved by abortion occurring about the sixth month. There is no history of cancer in her family. The catamenia appeared at the age of fourteen. She was often irregular, and frequently menstruated bi-monthly. Suffered from hæmoptysis and palpitation before marriage and since; has had rheumatic fever. Four years ago she began to suffer from severe lumbar and pelvic pain, and also menorrhagia; has often lost large quantities of blood; no other discharge; has vomited blood in rather large quantities during this gestation.

Tactile examination reveals a scirrhus-like induration of the cervix uteri, which is nodulated. One very hard nodule, larger than a hazel-nut, occupies the right side of the anterior lip and cervix; there are several smaller ones. The catamenia ceased on the 1st of May last; and as she was so reduced by the vomiting, Dr. Barnes, at her own earnest request, appointed Christmas-eve, at seven P.M., to induce premature labour, remarking at the time that it would not interfere with Christmas-day, as he could promise to terminate the labour in less than eight hours. It was all over in half that time, as the sequel will show.

At fifteen minutes past seven on that evening Dr. Barnes introduced a small caoutchouc dilator into the os uteri by means of the uterine sound. It would then barely admit the tip of the little finger. In half an hour, by water pressure, two fingers could be admitted, and the vagina was nicely dilated. The stop-cock was then turned off, and the instrument removed for a little while. A rather larger dilator being then introduced, another half hour had enlarged the os uteri to admit nearly three fingers, but dilatation went on very slowly in consequence of the scirrhus-like induration. Dr. Barnes then ruptured the membranes at half-past eight P.M., and incised the os uteri at several points with a guarded penknife. The morbid thickening and rigidity of the cervix were extreme, and would have been incompatible with the birth of a living child at term. Some pains followed, and the dilator was again introduced, and pretty fully distended with water. After about half an hour more the dilator was removed, and opportunity given to allow the induced uterine action to work. A little before ten P.M., the os uteri was sufficiently dilated to allow Dr. Barnes to adapt the long forceps, with which, in another half hour, he terminated the labour. The face was directed anteriorly. The placenta followed in a few minutes, and the uterus contracted well. The child, a boy of good size for eight months, was born semi-asphyxiated; but after half an hour's use of warm and cold water effusions, and Dr. Silvester's method of artificial respiration, it respired freely. The mother has not had a single bad symptom, and the child, though feeble, bids fair to live. Dr. Barnes hopes that besides relieving the vomiting (which it has done), this premature delivery may, by the mainmarty diversion keeping the uterine and ovarian organs in quiescence, afford opportunity for recruiting the mother's general health.

ART. 137.—*On Turning in Cases of Disproportion.*

By Dr. M'CLINTOCK.

(*Medical Times and Gazette*, Aug. 23, 1863.)

This paper, which was read at a meeting of the Obstetrical Society of London, embodies the results of seventeen cases which came under the care of Dr. M'Clintock in the wards of the Dublin Lying-

in Hospital. In each of them turning had been performed, at various periods after the commencement of labour, on account of disproportion between the head and pelvis. In none of them was there any considerable deformity of the pelvis, though the obstetric histories of the women clearly showed that there must have been some slight narrowing of the superior strait. More or less difficulty was experienced in every instance in bringing down the head into the pelvis, and twice craniotomy had to be resorted to. On one occasion the parietal bone (that next the sacrum) was fractured in pulling the head through the brim of the pelvis. With one exception, all the patients were deeply chloroformed before the operation of version was undertaken. Nine of the children—viz., four boys and five girls—survived birth, though all were alive when the operation was commenced. Of the eight children dead-born, five were boys and three girls. The heart continued to pulsate for several minutes after birth in some of the children recorded as “dead-born,” Dr. M’Clintock not considering a child as saved by an obstetric operation, nor recording it amongst the “live births,” unless respiration be fully established. All the women recovered satisfactorily but one, who died of puerperal fever, of which some cases had occurred at the time in the hospital. In reviewing these cases, Dr. M’Clintock expresses an opinion that the operation is not so favourable for the child as some of its advocates had supposed, and that it is only when the amount of pelvic narrowing was very slight that we could reckon with any degree of certainty upon saving the fœtus. He does not, therefore, recommend the operation in preference to the induction of premature labour in cases where an option was left us, and a decided contraction of the pelvis was known to exist. At the same time that it is a valuable resource in cases of this class which may have reached the full period of pregnancy, he proves by the fact, that of eighteen boys born to the above patients, and delivered by other modes than turning, only two were alive at birth; whereas four out of the nine delivered by turning survived their births. Looking to the interests of the mother, the author of the paper considers that the operation of turning in the particular class of cases under notice has stronger claims; for not only does it abridge the labour process, which in itself is no small advantage, but it averts the possible contingencies of craniotomy, high forceps operation, or even of rupture of the uterus. Its great mechanical advantage, Dr. M’Clintock thinks, is due, not to the position of the head nor its greater compressibility when coming through the pelvis with the base foremost, but to the unlimited amount of force which we can bring to the aid of the uterus by traction on the body of the child.

ART. 138.—*A Case of Painless Parturition.*

By Dr. GEORGE SMITH, of Madras.

(*Edinburgh Medical Journal*, November, 1862.)

The account of this case was read at one of the meetings of the

Obstetrical Society of Edinburgh; and after the reading more than one of the members present made statements which go to show that painless parturition is not so very uncommon an event as might be supposed. Dr. Pattison stated that he had once attended a primiparous patient who suffered no pain at all during labour. He had not been summoned to the case, but happened to call at the time; the child was born quite easily, the patient only experiencing a feeling of pressure. Dr. Wilson said that he had once been called to see a woman who had been delivered without any pain, whilst she was walking about in the house; and he found the child lying on the floor with the umbilical cord torn across; and Dr. Alexander B. Simpson stated that Von Ritgen, the venerable professor of midwifery at Giessen had told him, that in the long course of his practice he had met with no less than seventeen cases of labour where the patient had experienced none of the ordinary labour pains; and that he had been led to form the conclusion that in perfectly natural labour, pain should not necessarily be experienced, and that we had come to regard pain as a natural and necessary concomitant of labour, merely because women were almost never in a perfectly healthy condition when we were summoned to aid them during parturition. Dr. Simpson thought that if Professor Von Ritgen's position could be established—and the facility of parturition among savages went far to prove its truth—then the objection sometimes made to the use of chloroform in labour, on the ground of its being contrary to nature, would be most completely done away with.

CASE.—Some years ago I was engaged to attend an English lady during her approaching confinement, and was startled one day by a hasty summons, coupled with the information that the child had been suddenly born without warning of any kind. On reaching my patient's residence, I found that the child had been born about ten minutes, and that it was still lying, with the umbilical cord uncut, close to the mother's body. The native female servant, at the lady's order, had left the child untouched, merely raising the bedclothes a little to permit the free access of air for the purpose of respiration.

On inquiry, the lady informed me that she had been for some time expecting her confinement daily; that the previous night she had felt as usual; but that she had occasion to rise frequently to attend upon her sick child, and that she had got up as usual about half-past five A.M., feeling well, and having no indication of the near approach of labour. Further, that during the forenoon she had walked down a long flight of steps, and across a gravelled walk to a smaller house within the enclosure of her own grounds, where, feeling a little tired, she had lain down upon a bed—that soon after she experienced slight discomfort, likened by her to ill-defined uneasiness of the abdomen under the operation of a mild laxative, followed by an impression that some solid warm body was lying in contact with her person—that she directed her servant to look below the bedclothes, and that the attendant on doing so, found to her surprise the child entirely extruded.

My patient assured me repeatedly and earnestly that she was quite unconscious of the whole parturient process culminating in the birth of the child, and expressed herself both surprised and alarmed at a delivery so painless and instantaneous. As she was daily, nay, hourly, expecting her delivery, it is but reasonable to suppose that she had been for some time

acutely alive to the earliest intimations of commencing parturition, and it is surely remarkable that nothing occurred from which she could have suspected that the act had actually commenced. My patient had no object in deceiving me, and I am quite satisfied of the entire truthfulness of her often—to me—repeated statement.

This case has a medico-legal significance, as well as a practical. If a female awake, in perfect health, in the exercise of sound reason, and hourly expecting her confinement, having no object for its concealment, but many reasons for its occurrence being welcomed by her friends, can be the subject of painless, unconscious labour, preceded by no appreciable premonitory symptoms, and making itself known only when the extrusion of the child has been completed in the way described, how much more may we be inclined to yield belief to cases in which it has been averred that delivery has taken place during sleep, without waking the mother, and to others, in which it has been maintained that owing to the painlessness of the parturient process, the child's life had been lost by a fall on the ground, or by being engulfed in a latrine? The child was a female, small, but not much undersized. The mother's first labour--this was the second--was a normal one, accompanied by the usual signs, and extending over six hours in its duration.

ART. 139.—*Case of Sudden Parturition.*

By Dr. C. BLAKELEY BROWN.

(*British Medical Journal*, April 3, 1863.)

CASE.—Anne S., aged 26, began to feel ill about 3 o'clock P.M. on March 14th. While resting on the bed, she felt, as she thought, a "call of nature;" and, while sitting up, gave birth to a fine full-grown child, which falling on the floor, the funis was broken off about eleven inches from the navel. She took her child and lay down with it for some time; when, feeling easier, she dressed herself, went to the door of the house, hailed a cab which she saw passing, and getting into it with her baby, was driven to Queen Charlotte's Hospital. On her arrival there, she walked out of the cab up several steps with the child in her arms, before the doors of the hospital could be opened; and having entered, would have walked up stairs, if the matron had not insisted upon her being carried. This occurred between 8 and 9 P.M. When she was put to bed, the matron discovered that the placenta had not come away: this she removed without difficulty, and found the torn funis to correspond with that still on the child, which she tied and cut, and from which there had been no hæmorrhage whatever. Both mother and child are now in the convalescent ward, and have had no bad symptom, or seem to be any the worse for their journey.

ART. 140.—*Case of Acute Inversion of the Uterus.*

By Mr. R. T. THORNE.

(*Lancet*, March 7, 1863.)

CASE.—Mr. Thorne was called up during the night of the 19th of December, 1862, to render assistance at a midwifery case. It appears that Mary M.—, aged 35, the mother of six children, was taken in labour at

ten o'clock A.M. on the 18th, and after a natural and easy labour was delivered at half-past twelve A.M. on the 19th of a female living child. Some little hæmorrhage took place after the expulsion of the child, in consequence of which the attendant endeavoured to remove the placenta by traction at the cord about an hour before Mr. Thorne's arrival. The latter gentleman found the patient in an extremely exhausted state, evidently labouring under some aggravated nervous shock. Her face and lips were pale and exsanguine; there was a cold sweat on the forehead and face: she had just fainted and vomited, and on a hasty examination of the wrist no pulsation could be detected. He ordered her some cold brandy-and-water at once, and proceeded to ascertain the cause of the symptoms.

On the bed close to the vulva, in a pool of blood and numerous clots, lay the placenta, still partially adherent to some body within the vagina. On passing his hand into the vagina, he found the whole cavity of the pelvis, which was very capacious, occupied by a firm globular tumour, which was pressing forcibly on the perineum. No uterus was detected above the pubes by the hand applied over the abdominal parietes; but the extremities of the fingers of his hand within the vagina could easily be felt. He at once detached the placenta, which was then only adherent to the extent of about two square inches (the entire surface had, however, evidently been forcibly detached), and then with the back of his flexed fingers he endeavoured to replace the inverted organ, and after about four minutes' continual firm pressure it began to yield, and at last resumed its natural position, his hand occupying its cavity. The attendant and Mr. Thorne then kept up digital pressure over the uterus for about an hour and a half, on account of repeated oozings of blood, the patient taking during this time about half a pint of brandy and some infusion of ergot. She complained of great dizziness and of noises in her ears; but arterial action being re-established, and the hæmorrhage having entirely ceased, a firm pad was applied over the uterus, and left.

The next morning she felt much better, and had a far less anæmic appearance than on the previous night. She complained of great debility and great pain in the head, especially across the brow; skin warm and moist; pulse 68, full volume, very soft and compressible; tongue nearly clean. Has had no sleep.

From this time she gradually improved, taking nutritious diet and tonic medicines, and on the 3rd of January she was able to get up, and has since then gradually resumed her household duties.

ART. 141.—*A Case of Resuscitation in a Fætus after long-suspended Animation.*

By DR. S. CARTWRIGHT REED.

(*Lancet*; and *Pacific Medical and Surgical Journal*, Oct. 1862.)

CASE.—The following case is, perhaps, not an uncommon one; but I have never read of an instance in which life has been restored after so long a period as forty minutes after delivery, and more especially after the child has been pronounced dead by a surgeon. It is on account of the latter circumstance that I would present the particulars to the profession, with the object of cautioning others against falling into a similar error. I was summoned to a case of labour, but by some means the message was delayed, and another surgeon was sent for; the child was born before he arrived,

and upon examination, he pronounced it still-born, soon afterwards taking his departure. Twenty minutes after this I arrived, and found the family in deep sorrow for the supposed dead infant. I hastened to the bed-room, where the nurse was washing the body prior to laying it out on a table already prepared for that purpose. I determined, however, to examine the little thing ; and, placing my fingers over the region of the heart, I detected a fluttering, so slight that at first I hardly knew whether it was the pulsation in my own fingers or the feeble action of the child's heart. The surface of the body was of a dark livid colour, as also the lips, etc. I at once commenced forcing air through the mouth, by means of my own, into the chest, alternately inflating the lungs and depressing the ribs, taking care to compress the nostrils, and with my other hand slightly pressing the stomach to prevent the air entering that organ as much as possible. The body was enveloped in flannel and held before the fire. After continuing this forty minutes, it gave a sigh. I persevered twenty minutes more, after which time it began to breathe of itself, and was handed to the thankful mother.

ART. 142.—*On Nursing.*

By M. TROUSSEAU.

(*Journal of Practical Medicine and Surgery*, May 3, 1863.)

All mothers (said M. Trousseau, in a clinical lecture recently delivered at the Hôtel Dieu, in Paris), cannot suckle their children ; some are prevented by the state of their health, others by social duties of an imperative description, and a nurse must be employed. The physician should consequently be able to select a woman fit for the task, a matter of some difficulty on account of the erroneous views entertained on the subject by the public, and of the class of persons who devote themselves to this occupation.

A handsome face and good teeth are of secondary importance ; but a good nurse should be young and have already reared one child at least. Thus, said M. Trousseau, a cow at the third calving will yield a more copious supply of milk ; and a woman will also probably prove a better nurse if she has already suckled one or two infants, in which case both the breast and nipple are more fully developed. When, on the other hand, phlegmonous inflammation of the mamma has previously occurred, it is presumable that a more or less considerable portion of the gland has suffered in its texture, and that its secreting power is impaired, and, moreover, that a repetition of the abscess may supervene in the course of any future lactation. Whereas, if, on the contrary, she has suckled one or two children without any such mischance, it may safely be assumed that she will be preserved from similar contingencies.

The nurse should have been confined six weeks or two months, because chaps of the nipple and the subsequent phlegmons generally appear within that time.

A full, rounded form of the breast is by no means indispensable ; this may be chiefly dependent on adipose development, especially if the subject is generally inclined to plumpness ; a *pyriform* shape is

preferable, and on superficial palpation, an hour or two after suckling, nodosities should be observable, due to the accumulation of milk.

The contact of the child's mouth, of a soft hand, should cause the nipple to enter into an active state of erection, and on slight pressure the milk should gush forth to a distance of four or five feet from the orifices of five, six, or eight lactiferous ducts. A blue network of enlarged veins should moreover be discernible beneath the transparent texture of the skin.

After these remarks on the external appearance of the secreting organ, M. Trousseau expatiated on the characters and qualities to be sought for in the milk.

When milk is drawn from the cow, the supply presents different characters during the three successive periods of the operation. The milk, of a bluish tinge at first, chiefly consists of whey or serum, with a small addition of casein, and no butter. In the second period, the secretion is white, contains less whey, a larger proportion of casein, and very little butter. The fluid, during the third stage, acquires a yellow colour due to the predominance of butter, some casein is present, and the amount of whey is considerably diminished. Cow-keepers are well aware of these facts, and take advantage of their knowledge for the purposes of their trade.

In the same manner, a nurse examined three hours after lactation will yield milk at first serous and bluish, subsequently white, and finally yellow. But we possess but one trustworthy method of ascertaining with accuracy the amount of the supply—*i.e.*, weighing the infant before and immediately after it has been suckled. Professor N. Guillot has thus discovered that a nursling draws when put to the breast, an ounce and a half or two ounces of milk during the first month of life, from eight to ten ounces during the third month, and between thirteen and sixteen ounces when he has reached twelve months.

It is highly desirable that a wet-nurse should be preserved as much as possible from all causes of physical or mental excitement calculated to affect the lacteal secretion in its amount or quality. Thus, to a hired nurse sexual intercourse should be entirely prohibited, and allowed but with much caution to a mother who suckles her own child. We learn, it is true, from comparative physiology, that brood-mares are again led to the stallion nine days after parturition, and that cows which in the course of six weeks or three months after calving are not in heat, are considered unlikely to yield a good supply, and although impregnated again, continue to nurse the calf sufficiently. It must, however, be acknowledged that the coincidence of pregnancy with lactation, promptly causes a diminution of the amount of the milk, although the qualities of the fluid remain unchanged.

A good nurse should not menstruate for eight months; if the catamenia return at an earlier period, the circumstance should, however, inspire no alarm, although during the menses the milk will contain a larger proportion of colostrum, which may induce a slight disposition to diarrhœa in the infant; but if the child's health

is otherwise satisfactory, this little disturbance will prove unimportant, and spontaneously subsides after the cessation of the monthly period.

Many erroneous opinions prevail as to the kind of food most appropriate for nurses: they should, in general, abstain from stimulants; but, with this exception, their diet should be that of the family whose service they have entered.

M. Trousseau, alluding to the popular prejudice against a mother suckling her infant before milk fever has set in, observed that all animals suckle their offspring immediately after parturition, and women who adopt this plan find it advantageous, because the first acts of suckling form and develop the nipple, before fever and the rising of the milk tumefy the breast and diminish the mamillary prominence. The longest and largest nipple is the best. Now, the braces used by young girls interfere with the development of the nipple far more than stays supplied with gussets, in which the breast can readily expand. This must be borne in mind, and young women desirous of nursing should be advised when they have reached the sixth or seventh month of pregnancy, to ascertain the size and condition of the nipple. If the organ is insufficiently developed, an artificial box-wood or ivory teat should be applied over the areola, and the nipple will promptly acquire the desirable size.

If the child and mother both prosper during the process of lactation, no supplementary food need be exhibited up to the end of the fourth month. The healthy appearance of the mother must not, however, be considered as positive evidence of a perfectly sound condition of her system, for at this period gastric pain and dyspepsia often supervene, rapid loss of flesh follows temporary embonpoint, and diminution of the lacteal secretion is the consequence.

On the other hand, in the case of a hired nurse, whose milk, at the time of her first entering on her duties, was already four or five months old, it will tend to decrease in quantity when the nursing enters on its fifth month, nine or ten months having then elapsed since the date of the nurse's confinement. Hence the necessity of additional nutriment for an infant of four or five months, and sometimes even younger.

The supplemental food should, according to M. Trousseau, consist of cow's or goat's milk, diluted at first with equal parts of toast or barley-water, and subsequently with one-third or one-fourth only. It should be slightly sweetened, but always be less sweet than the mother's milk, in order that the nursing may not lose its taste for the breast. The infant should then be suckled three times only in the day—early in the morning, at noon, and in the evening—and the contents of two or three sucking-bottles may be exhausted during the night, in order that the nurse or mother may enjoy undisturbed repose. Carefully prepared and well-boiled pap may then be resorted to, and, despite the denunciations of J. J. Rousseau, this kind of food is preferable to panada, and more especially to the various farinaceous succedanea advertised in the newspapers, and which families consider themselves obliged to purchase. After some time light meat soup may be tried.

M. Trousseau advisedly used this expression *tried*, because in the rearing of a child the mother must feel her way, and proceed with much caution. Each infant, he said, had its own peculiar idiosyncrasy, and some have been known incapable of digesting any kind of milk, even the mother's. Supplementary milk may, therefore, be rejected. Soft farinaceous food may then be substituted, such as concentrated infusions of toast, barley or oats, light pudding, soups, and underdone fresh eggs. In Burgundy and Normandy, toast boiled in wine or cider is exhibited without any apparent injury. The amount of this kind of substitute should be gradually increased in proportion as the period of weaning draws near, in order to prepare the child for that trying process.

ART. 143.—*The Removal of a Living Child from a Dead Mother.*

By Dr. W. M. TURNBULL, Physician to the Melbourne Lying-in Hospital.

(*Australian Medical Journal*, October, 1862.)

CASE.—On visiting the hospital on the 27th inst. (Sept.), I was informed by the resident surgeon, Dr. Fetherston, that Mrs. C. had had a fit and was dying. I immediately proceeded to the ward in which she lay, and found her dead. Having fully satisfied myself that such was the case, I had her body removed to another room, and immediately made an incision through the parietes into the womb, and extracted the child (a female). The cord still pulsated, justifying the remark made by Burns, that "The uterus may live longer than the body, and after the mother has been quite dead, the child still continue its functions." The child shortly after birth gave a slight inspiration, repeated at considerable intervals. The pulsation in the cord having ceased, the child was removed, and by the assistance of alternate hot and cold baths, along with the "ready method," respiration was fully established. It only lived about four hours—during which time it cried freely and swallowed a little milk twice. Mrs. C. was only between six and seven months gone in pregnancy.

ART. 144.—*A Strange Case of Extra-uterine Pregnancy.*

By Dr. MÜLLER.

(*Allgem. Wien. Med. Zeitung*, 1862; and *Med. Chir. Rev.*, January, 1863.)

Dr. Müller relates a case in which the ovum had become developed in an inguinal hernia of the right side, so that the sac springing from the inguinal region formed a round tumour reaching to the knees, and weighing about eight pounds. By operation a mature living child was brought to light. The mother died an hour afterwards of internal hæmorrhage. Post-mortem examination was not permitted.

ART. 145.—*A Curious Case of Extra-uterine Pregnancy.*

By Dr. BRANDT, of Madeira.

(Edinburgh Medical Journal, Sept. 1862.)

This case is taken from the Proceedings of the Obstetric Society of Edinburgh.

CASE.—Francisca Amelia Vieira was born in the island of Madeira in the year 1778. Was married to Alexander Vieira in 1795. Was confined of her first child (a son) on the 20th of September, 1796, who died in London on the 31st March, 1802, aged five years, six months, and ten days. Five years after the first confinement she had a daughter named Maria, who was baptized at home, and died soon after. Three years after this she became in the family way, but was never confined of it. Four years after she was confined of a son named Ernesto, who is still living in Italy; still remaining in the family way of the third child. Seven years after the birth of Ernesto, she was confined of a daughter, who is also still living, called Amalia Augusta Vieira. She died on the 7th September, 1858. On the 8th September, 1858, at 8 o'clock A.M., the post-mortem examination was made on the body of Francisca Amelia Vieira, in the presence of Dr. Juvenal Osorio de Ornellas, Surgeons Joao Nepomeceno Gomez and Francisco Simplicio Lomelino, Henry Crawford, the head nurse, several students and chemists, and the servants of the hospital. A bony tumour was extracted from the abdomen, on the right side of the uterus in the Fallopian tubes of that side; the uterus, and Fallopian tubes on the left side were perfect. The tumour weighed 4 pounds, was 8 inches in length, $5\frac{1}{2}$ inches in diameter, and 16 inches in circumference. The tumour was divided longitudinally with a saw, as near the centre line as possible.

The two parts of the tumour which I saw in April, 1861, were very much disfigured, being badly preserved in a dry state, and almost crumbling to pieces.

Francisco Amelia Vieira was born in 1778.

Married	in	1795	. . .	17 years old.	
First child	"	1796	. . .	18	Son.
Second child	"	1801	. . .	23	Daughter.
Pregnant	"	1804	. . .	26	Not born.
Third child	"	1808	. . .	30	Son living.
Fourth child	"	1815	. . .	37	Daughter living.
Died	"	1858	. . .	80	

Was pregnant of the third conception fifty-four years.

The tumour or bony cyst contained a foetus, which, from certain signs which will hereafter be mentioned, must have lived a long time after the natural term of birth. Its position in the bony case was, head uppermost, looking to the left and downwards, spine and back lying against the right side of the cavity, the nates occupied the inferior part of the cavity, the thighs and legs turned up, so that the feet were next the head; the whole body was twisted. It so happened that the saw cut through the head and body, dividing it through the median line; but as the body was twisted, the section could not show two equal sides.

The head, in consequence of the pressure exercised on it by the cyst, was crushed in such a way that the parietal bones *passed over* the occipital and part of the left temporal bones, and *under* the posterior border of the frontal bone; from appearances the whole of the cranium was ossified.

The upper jaw of the larger half of the head has three teeth, one of which appears to be the second molar; the depressions of these teeth can be seen on the corresponding side of the other half of the head.

The two inferior extremities and the right arm and hand are placed between the right side of the head and the parietes of the cyst, in a very compressed state. The left arm and hand are situated on the opposite side of the head, between this and the corresponding side of the cyst, also in a compressed state.

Part of the umbilical cord can be traced round the neck, along the right side of the face and body, with the inferior members, to the navel. The instep of the left foot is placed against the chin and mouth, there forming a depression. From the different positions of parts above mentioned, it is clear that the whole body from the neck downwards is twisted from left to right, and the inferior members are turned upwards along the right side of the trunk.

The right knee lies on the mastoid process (right); the right foot lies in the following way: External border on sutura sagittalis. Heel, on the malar bone, right side. Sole, part on the right side of the frontal, and part on the temporal bone (right). The left femur, which was cut longitudinally by the saw, presents a small medullar cavity; the compact tissue is extremely hard, and has the appearance of ivory (eburné). On the condyles of the tibia can be seen remains of the epiphysarian cartilage; the left omoplate, which was also sawed, is extremely hard. The liver is excessively large; part of the intestines are visible.

(B) CONCERNING THE DISEASES OF WOMEN.

ART. 146.—*On the Recent Epidemic of Puerperal Fever in Dublin.*

By Dr. DENHAM, Master of the Dublin Lying-in Hospital.

(*Dublin Quarterly Journal of Medical Science*, Nov. 1862.)

The fever described by Dr. Denham in this memoir is asthenic in type, and with a "clear relationship" to erysipelas and scarlatina. With respect to the contagiousness of the disorder, and the general characteristics of the attack, Dr. Denham says:—

"I beg not to be understood as wishing it to be inferred from what I have said, that a disease is not contagious merely because it prevails epidemically; the two may and do meet in the same malady, and must not, therefore, be regarded as incompatible properties; and I am free to admit, that those properties may often be found to combine in the very disease of which we are now speaking. What I complain of is, that those writers who advocate the infectious nature of puerperal fevers, in their anxiety to establish their position have entirely overlooked the epidemic element, and have heaped unmerited reproach on medical men who have had the misfortune to live within the epidemic circle, and upon institutions that have been the recipients of patients who have imbibed the epidemic poison perhaps for days before they entered within its walls.

"In the month of November last there were 72 patients delivered

in the hospitals; 16 were attacked with puerperal fever, and one with scarlatina; she recovered. Of the 16 puerperal cases nine died; in five of them the fever was of a low typhoid form, proving fatal on the fourth or fifth day after delivery. In three of them the symptoms presented were those of uterine phlebitis and pyemia—proving fatal—one on the seventh, one on the eighth, and one on the ninth day. The remaining fatal case was attacked with pain (abdominal) and rigors on the second day after delivery; on the night of the fifth day she had two marked fits of convulsions, with vomiting of black fluid; she never became conscious after the first fit, but lay in a state of stupor until the ninth day, when she gradually sank and died.

“One of the six non-fatal cases was attacked with pain and swelling of the left knee, which extended to the whole leg, causing much pain and constitutional disturbance. Another had, superadded to the phlebitic symptoms, erysipelas of her left elbow, ending in abscess; she became maniacal on the seventeenth day, and was removed by her friends on the twenty-first, contrary to our wishes.

“In December there were 103 deliveries—and of these 14 were attacked with puerperal fever, and three with scarlatina. Nine of the puerperal cases proved fatal—death taking place in all, with one exception, from the fourth to the sixth day. In the exceptional case, the urgent symptoms so far subsided as to enable the patient to be removed to the recovery ward. There she was attacked with phlegmasia dolens, inflammation of the knee-joint, and sloughing over the sacrum and on both hips. She was released by death from her intense and prolonged sufferings on the thirty-sixth day after delivery, every texture in the body giving evidence of blood-poisoning to a fearful extent. The patients attacked with scarlatina all died. One of the five cases not fatal, suffered from a severe attack of bronchitis, for which she was attended by Dr. Croker, our consulting physician; another had an abscess over one scapula.

“The new year, which to most people is a time of rejoicing, proved a season of deep sorrow and anxiety. From the 1st of January till the 11th, when the hospital was closed, we had 37 patients delivered. Of these, four were attacked with scarlatina, and 16 with puerperal. All the scarlatina patients died—two on the third day after the attack, and two on the fourth. Ten of the puerperal cases proved fatal; one of them died in 24 hours; another, in which there was morbid adhesion of the placenta, died on the third day, with symptoms of uterine phlebitis; in another pleuritis was superadded to the phlebitic inflammation, and proved fatal on the seventh day. Two others were attacked with erysipelas and died—one on the twelfth, and the other on the sixteenth day.

“Of the six cases that recovered, one was attacked with erysipelas of the right arm, which ended in an abscess.

“The hospital was again opened early in the month of February, admitting, however, only a limited number of patients. Thirty deliveries took place; and of these six were attacked with puerperal

fever; two of them died, and one was in hospital until lately—a case of considerable interest.

“She appeared a healthy, active girl, and had a safe and easy labour for a first child. She became maniacal, however, on the second day, having had a rigor a few hours before; symptoms of metritis and peritonitis, with frequent rigors, followed; she complained much of pain in the head; and on the tenth day inflammation of the right eye set in suddenly, with loss of vision; an erysipelatous patch also appeared on the right elbow, which ended in a small abscess. Sloughing over the nates next appeared; but she ultimately recovered, with loss of vision in the right eye.

“In this case the head was shaved, and nauseating doses of tartar emetic were given in the first instance; when the pain set in over the uterus she was leeches, as the pulse was quick and strong. She was then put on hypo-sulphite of soda in scruple doses; but this we were obliged to discontinue, as it induced vomiting and purging, although given with small doses of liquor opii; bark and carbonate of ammonia were then given with a liberal allowance of wine and chicken-broth. I should have mentioned that the chest was much engaged in this case also. For many weeks she remained in a most pitiable condition, but has now left quite well.

“From the brief and imperfect report now given it will be seen that out of 150 cases 50 were attacked with puerperal fever, and eight with scarlatina; 20 of the puerperal cases died, and seven of those attacked with scarlatina; of the latter, one lived till the fifteenth day; the case was one of great interest, but very unpromising from the commencement; eruption very abundant, but of a dark livid colour; pulse weak and rapid; sordes on the teeth; great prostration, with low muttering delirium; yet, contrary to our expectation, the fever subsided, the pulse fell, and desquamation extensively set in, giving us a strong hope of recovery, when, unfortunately, an overloading of the stomach with food, incautiously given by the nurse, brought on an attack of vomiting, under which she rapidly sank and died.

“Bark, with carb. ammonia, brandy in small quantities, and wine to the amount of 148 oz., formed the principal treatment in this case.

“The class of patients that came in at this time were ill-fed, ill-clothed, and often depressed, not only in body but in mind; some of them unmarried, and some of intemperate habits; very much the class of persons that fell victims to the cholera when it prevailed in this country. The cholera in India is much more frequent and fatal among our common soldiers than their officers; and so with puerperal in this country, be it infectious or epidemic: it is not only more general, but much more fatal among the poor and the outcast than among those who are well to do in their comfortable homes. As I have already mentioned, the present outbreak was characterized by the early and urgent appearance of the symptoms, and the little influence exerted upon them by treatment. The data given will show how short the time allowed us to attempt a cure,

and how unpromising the material we had to work upon. Depletion could not be borne; and, when tried, did not seem to allay the symptoms. Stimulants and light nutritious food were what we relied most on, and apparently with most success. To some we gave muriated tincture of iron; to others, the hypo-sulphites; and others again we treated with camphor and Dover's powder. But the result appeared to be much the same in all. Our success, or rather the absence of it, was quite as great in the one form of treatment, so far as specifics were concerned, as the other; and the conclusion has been forced upon us that, as in cholera, so in puerperal, we have yet to seek for a certain remedy.

"One patient had a rigor 14 hours after delivery, and died on the fourth day. Another in 16 hours after—she died on the fourth day also. A third shivered 17 hours after delivery, and died on the fifth day. And a fourth in 20 hours after—she only survived 36 hours.

"The amount of stimulants necessary was well exemplified in the case of Mary Clive, who took 164 oz. of wine during the attack—she recovered."

Dr. Denham then gives some interesting quotations from various authors on the disputed questions relative to the causation of puerperal fever, and concludes as follows:—

"If we admit that there is in all such diseases, whether epidemic or infectious, a latent period, and take into consideration the ordinary period of incubation in small-pox, scarlatina, and such like diseases; and bear in mind, at the same time, the fact that the symptoms of puerperal fever are often present even before labour comes on, or develop themselves in a few hours after delivery; I think we have forced upon us the conclusion that the poison has not been communicated by the fingers of the medical attendant or that of the pupil on duty, nor yet has it come from the walls of the hospital. In my opinion, the poison is often taken into the system (however communicated) perhaps for days before labour sets in, and there lies dormant, being thus kept in this latent condition by some law of the system, the result of the pregnant condition.

"May we not, therefore, fairly infer that puerperal fever possesses quite as much, if not more, of the epidemic as of the infectious character, and that we have yet much to learn, both as to the nature of the disease, and the best mode of treating it?"

ART. 147.—*On Ergot of Rye in Menorrhagia.*

By Dr. GRAILY HEWITT, Physician to the British Lying-in Hospital, &c.

(*Lancet*, Dec. 13, 1862.)

The great therapeutic value of ergot of rye in the treatment of cases of severe menorrhagia has scarcely received that amount of recognition which is its due. In very many cases of profuse men-

struation which come before us a treatment conducted on general principles is for the most part successful: we improve the hygienic condition of the patient, and make such alterations in her habits or mode of life as appear to be necessary. In other cases, where the profuse menstruation is dependent on or associated with actual disease of the uterus, presence of polypi, &c., the indications for treatment are of a different character. In some instances, however, the patient before us is the subject of a profuse loss of blood, the sole discoverable alteration of the uterus present being a lax, uncontracted, atonic condition of the organ; and a gentle but continuous oozing of blood is taking place, for the arrest of which we require a therapeutic agent quick and certain in its action, and which is capable of inducing the uterine tissue to contract. Such an agent we have in the ergot of rye. After making trial of its effects in several cases of the kind above alluded to, and having compared the results with those obtained after the use of other remedies, and especially of digitalis, Dr. Hewitt has come to the conclusion that the ergot is far preferable, and that in pure, uncomplicated cases of menorrhagia it is the best remedy in our possession.

The following case exemplifies, as fully as one case taken by itself can do, the good effects of the remedy in question:—

CASE.—Mrs. M'G—, aged forty-three, a rather stout woman, of good conformation, came under my care (acting for Dr. Tyler Smith) as an out-patient of St. Mary's Hospital in October last. She stated that she was the mother of thirteen children, the youngest of whom was born a year and a half ago. Menstruation was quite regular up to six weeks ago. The discharge then came on as usual, and lasted for three days. During the night she was called up by a knocking at the door, and imprudently walked over the cold steps to the door, and back to her bed. The discharge was thereby suddenly arrested for a few hours, and then there occurred a very severe flooding. Ever since that time she has continued to lose blood every day, and during every hour of the day, the quantity lost being more or less great at different times. No effect has been produced by the treatment to which she has been subjected, and she is now excessively pallid and anæmic, and so weak that she is hardly able to walk. She complains also of a giddiness in the head, singing in the ears, and imperfect vision. On examination, I found blood oozing slowly from the uterus; the os rather soft and wide open, and the whole organ evidently in a very relaxed atonic condition. She was ordered to remain in bed; to apply cold to the hypogastric region; to take a little brandy-and-water and beef-tea occasionally; to drink lemonade, and to take night and morning one tea-spoonful of ergot of rye in powder, mixed with a little boiling water. Three days later she sent to the hospital to say that she was much better; that the hæmorrhage had been arrested after the first dose of the medicine, and she was desirous of knowing whether she was to continue it, as every dose "made her so sick." The patient had lost so much blood that it was evident further loss might be attended with danger to life, and it was hardly anticipated that so immediate an arrest of the bleeding, which had been going on for six weeks, would have been procured.

The action of the ergot in determining the cessation of the hæmorrhage in these cases is probably dependent on the contraction

of the uterine tissue to which its administration gives rise; it is, in fact, analogous to the action of ergot when given to arouse uterine action in cases of inertia of the uterus, hæmorrhage after labour, &c. The condition of the uterus in a long-standing case of menorrhagia, such as that above related, is one almost completely identical with that of the uterus after an abortion; the walls are thick and full of blood, and the hæmorrhage continues simply because there is an absence of any tendency to contract. That contraction is produced by the ergot. There does not appear to be any evidence that, apart from its contraction-inducing power, the ergot has any special hæmostatic effect such as has been attributed to it. Some authors have objected to the above theory of the action of ergot in these cases, that in the unimpregnated uterus no contraction, properly speaking, can be said to take place. To this it is sufficient to reply, that if the presence of pains, sharp, short, and like miniature representations of the pains of labour, can be said to be a proof of the occurrence of uterine contractions, then we have proofs in sufficient number. One part of the action of ergot in these particular cases is to give rise to such pains—uterine colic, as they are appropriately termed by continental writers.

The fact that each dose of the ergot gave rise to vomiting in the case above related is worthy of notice.

ART. 148.—*On the Ergot of Rye and the Nature of its Action on the Uterus.*

By DR. GRAILY HEWITT, Physician to the British Lying-in Hospital, Lecturer on Midwifery and Diseases of Women and Children at St. Mary's Hospital Medical School.

(*Lancet*, Jan. 17, 1863.)

It may be taken as an undoubted fact, that the ergot of rye is very frequently found to produce a marked effect in exciting contractions of the uterus. That it not uncommonly is entirely useless when administered for the purpose of exciting uterine action, must also be admitted to be a fact. These two propositions imply either an absolute uncertainty in the action of the drug, or the presence of a peculiar idiosyncrasy in particular individuals, one set of patients being liable to be affected with ergot, and another not so liable. The only other alternative is to suppose that there is a want of potency in the particular specimens used; but the answer to this is, that particular specimens under the same conditions as regards freshness, physical condition, and otherwise, have been found to vary in their effects as above described. To adopt the second alternative—that particular individuals have special idiosyncrasies as regards the action of ergot, is hardly a satisfactory mode of explaining the matter.

On what, then, depends the fact that ergot is at one time active, and at another time without apparent action?

To answer this question, we must first endeavour to ascertain what is the nature of the action of ergot in inducing contraction of the uterine muscular tissue, or in lessening the bulk of the organ. This question is one of the greatest possible interest and importance; but it is one which has not yet received a satisfactory answer.

"Some little time since, while engaged in studying the subject of puerperal fever, and in tracing the connexion—a connexion the existence of which appears to be substantiated in a certain number of cases—between defective contraction of the uterus after delivery and the occurrence of this disease,—I was struck with the fact that in so many cases ipecacuanha had been reported as having been exceedingly efficacious in preventing the outbreak of the disease, or in checking its progress at the early stage; while, as I knew from other sources, the same drug was also found to possess great efficacy in restraining uterine hæmorrhage. It immediately occurred to me that this action of ipecacuanha in restraining uterine hæmorrhage, and in preventing puerperal fever (and which was, doubtless, in both cases due to its action in exciting contraction of the uterus), must be connected with the vomiting excited when the drug was taken into the stomach. Thus far, then, the argument was: ipecacuanha produces contraction of the uterus, and ipecacuanha produces vomiting; probably, therefore, the contraction of the uterus is dependent wholly or chiefly on the vomiting. Considering next the action of ergot in producing contraction of the uterus, and being aware how frequently ergot, when given, sets up vomiting or occasions nausea, I was led to adopt the conclusion which it is now my object to enunciate: that the action of ergot, as an excitor of uterine contraction, is dependent on a like cause—viz., the vomiting and nausea often set up by its administration.

"Such were the steps by which I was induced to adopt a theory as to the action of ergot which I have since found to be borne out by the facts which have come under my own observation. The theory is one the truth or untruth of which can be easily determined by a sufficiently large number of well-observed facts. I regret that the observations at this moment in my possession are insufficient in number to give them any great scientific value in the determination of this question, although they appear to myself to be sufficiently convincing to warrant this my expression of belief on the matter.

"The facts necessary to be observed are: 1. Ergot being given in a particular case, and a marked ergotic effect being produced, is this effect co-existent, or not, with vomiting or nausea? 2. Ergot being given, and no such marked ergotic effect induced, is there an absence of vomiting and nausea? (It would be advantageous to have a number of observations under this head.) 3. Ipecacuanha or any other emetic being given, are the contractions of the uterus intensified thereby, or increased in frequency? Considerable care would be necessary in recording the answers to these questions, to give the observations a true scientific value.

"Some very important results would follow the adoption of the theory now contended for in obstetric practice, and the question which was put at the commencement of this paper—"On what is

the uncertainty of the action of ergot dependent?'—would receive, in all probability, a satisfactory answer. I am quite aware that, even admitting that the above explanation is so far a true one, this would only be an advance on the road to a perfect explanation of the matter: the whole relation of vomiting to uterine action and activity will have still to be considered. It is well known that uterine action itself very frequently excites vomiting; and it may be said, therefore, that, admitting that ergot acts only when it excites vomiting, this vomiting is not so much the effect of the ergot as an evidence of uterine activity having been excited. The physiological and the clinical elements in the consideration may, however, be very usefully separated. Clinically, it may be, as I believe will be, substantiated, that ergot given without producing vomiting will generally be found useless as a parturifacient. What the explanation of the connexion between the vomiting and the uterine action may be is another question, which must be discussed separately."

ART. 149.—*On Effusions of Blood in the Neighbourhood of the Uterus, or the so-called Peri-uterine Hæmatocele.*

By Dr. HENRY M. TUCKWELL, late Radcliffe Travelling Fellow.

(Pamphlet: Oxford and London: Parker. 1863. pp. 41.)

This pamphlet, which is a thesis for the M.D. degree at Oxford, is in every sense deserving of attention. It is a model of what a monograph ought to be—clear, concise, and yet rich in detail. If, indeed, it contained nothing but the concluding tabular view of cases, no less than ninety-six in number, it would have enough to recommend it to favourable notice.

ART. 150.—*A Case of Multiple Medullary Cancer complicated with Pregnancy.*

By Dr. TANNER, Assistant-Physician for Diseases of Women and Children to King's College Hospital, &c.

(*Lancet*, Jan. 2, 1863.)

CASE.—On the 29th of July, 1862, I was requested by Dr. Thane, of Hart-street, Bloomsbury, to see with him Mrs. C. L—, aged thirty-nine, residing in Drury-lane. The poor woman was the wife of a very respectable working man, and was in comfortable circumstances for her station in life. She had always enjoyed remarkably good health. None of her relatives had ever suffered from cancer; but there was probably a slight family tendency to phthisis—that is to say, all her half-brother's children

had died from pulmonary consumption. She had been married ten years ; and was about five calendar months advanced in her third pregnancy. The first child had been born on the 12th of October, 1854. While suckling this infant pregnancy again took place, and on weaning the child at the beginning of 1856, she aborted, being three months advanced in gestation. After this accident the general health continued very good until the 2nd of May, 1861, when her child died from an attack of croup ; and she was rendered so miserable by the loss that she may be said to have been neither happy nor well since. The catamenia had been quite regular until the 25th of February, 1862 ; but this was the last day of their appearance.

At the commencement of the present year (1862) a small swelling, about the size of a hazel-nut, was first discovered in the abdominal walls, just to the right of the umbilicus. There was no other symptom of disease appreciable to her husband or herself at this time. The growth gave rise to no annoyance until a few weeks ago, when it commenced enlarging, and became the seat of lancinating pains. Similar swellings also then began to form in other parts.

On the 29th of July, at my first visit, I found her much reduced in flesh and strength. The appetite was good ; but as she suffered much from indigestion and flatulence, she was afraid to eat, although the remedies prescribed by Dr. Thane had given great relief. The sleep at night was very disturbed. In the abdominal parietes, just to the right of the umbilicus, there was a well-defined tumour, about the size of a large walnut. Adjoining the lower part of the ensiform cartilage of the sternum there was a more extensive mass, also seated in the abdominal wall. In the left groin there was likewise a flattened growth, about two inches and a half in diameter. On examining per vaginam, a very firm growth was discovered projecting into the canal, feeling as if it had its origin from the lower part of the sacrum, though in reality it was entirely seated in the recto-vaginal septum. It was clear that this mass would soon block up the vagina, and Dr. Thane assured me that it had much increased in size during the last fortnight. As before mentioned, she was five months advanced in pregnancy, and both the uterine souffle and the fetal heart could be plainly distinguished. The liver was much enlarged, and this enlargement, combined with the size of the uterus, occasioned a wearying feeling of distension, as well as some dyspnoea.

Taking all the foregoing circumstances into consideration, it seemed certain that the poor woman's comfort would be increased, even if life were not prolonged, by inducing premature labour—a proceeding which did not appear contraindicated by any feeling for the child, as it was certain that a live infant could not be given birth to through the natural passages. The propriety of waiting and ultimately effecting delivery by the Cæsarean section was discussed ; but such a plan of treatment was regarded as not applicable to the present instance.

Consequently on the 30th of July the membranes were punctured, and about half a pint of liquor amnii withdrawn. On the 1st of August, labour pains of fair severity set in ; but the os uteri was still very rigid on the following afternoon, and as the patient was getting tired and exhausted, and moreover as the mass of cancer in the recto-vaginal septum reached to within almost an inch of the pubes, it was evident that nothing could be gained by further delay. I therefore slowly tried to dilate the os uteri ; and this being partially effected, the fœtus was broken up with a pair of forceps, and removed piecemeal. No difficulty was experienced with the placenta ; the discharge of blood was very slight ; no injury was done to the uterus or vaginal walls, and the poor woman was left tolerably comfortable.

For some few days she continued to progress favourably ; but on the 10th August a severe attack of diarrhoea set in, aphthæ formed on the tongue and gums, and for the first time in her life she became jaundiced. It would only be tedious to give an account of the way in which she daily lost ground ; suffice it to say that matters gradually progressed from bad to worse until the morning of the 26th of August, when death took place from exhaustion.

At the autopsy, fourteen hours afterwards, the body was found greatly emaciated. In the abdominal parietes, to the right of the umbilicus, there was a deposit of firm medullary cancer the size of a walnut, together with a larger growth just below the ensiform cartilage. These were separate tumours, and not infiltrations. The liver was enlarged to about three times its natural size, and studded with medullary masses, varying in size from a pea to a small orange. There were likewise several deposits of cancer in the great omentum, in the spleen, in the walls of the colon, and one large mass binding the sigmoid flexure of the colon to the tissues of the pelvis. In the pelvic cavity there was a separate deposit, extending chiefly down the recto-vaginal septum, and completely blocking up the vagina. The inguinal glands on both sides were enlarged and infiltrated with medullary cancer. The uterus, considering the time which had elapsed since delivery, was of its normal size, and healthy. At the apex of the left lung there was a small deposit of tubercle, which had undergone calcareous degeneration, while at the same part of the right lung there were several small tubercles. The heart was healthy, but its walls were flabby. The kidneys were healthy.

ART. 151.—*Adhesion Clam: a New Instrument for Aiding the Removal of Ovarian Tumours, &c.*

By MR. JOHN CLAY, of Birmingham.

(*Medical Times and Gazette*, June 21, 1862.)

The letters refer to the same parts of the instrument in both figures ; *d* in Fig. 2 is intended to represent an adhesion.

The female blade is first passed underneath the structure to be divided, the end of the male blade is then inserted in the notch of the female blade and closed on the structure, which is thus included between the blades of the instrument. When the clam is closed, the male blade being narrower than the female, a ledge or groove is formed on each side of this portion of the instrument, in which the cautery moves in dividing the structure.

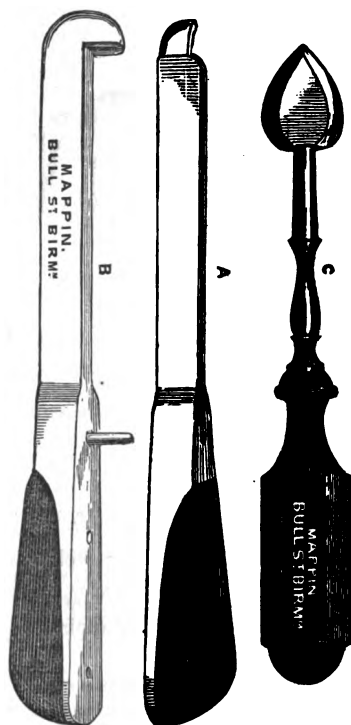
The cauterising iron may be used either cold or heated. In the former case the structure is rubbed through, and it acts, probably, on the principle of the *écraseur*, without producing the great tension of the parts which that instrument sometimes does. If heated, it is used in the same manner. The flame of a spirit-lamp is sufficient to impart to the iron the necessary heat.

One great feature in the use of this instrument is the great compression that can be used previously to the division of the structure, and Mr. Clay believes that this, with the use of either the actual cautery, or the friction instrument, is sufficient to prevent any hæmorrhage taking place.

It is probable that hereafter it may be found of service in dividing some forms of pedicles of ovarian tumours.

The recent discussion of the Obstetrical Society respecting the treatment of adhesions, the risk of hæmorrhage from these, and of lacerated omentum, and the large amount of traction produced on the structures by the *écraseur*, led Mr. Clay to consider that some different treatment was required in this part of the operation, and to design this simple instrument.

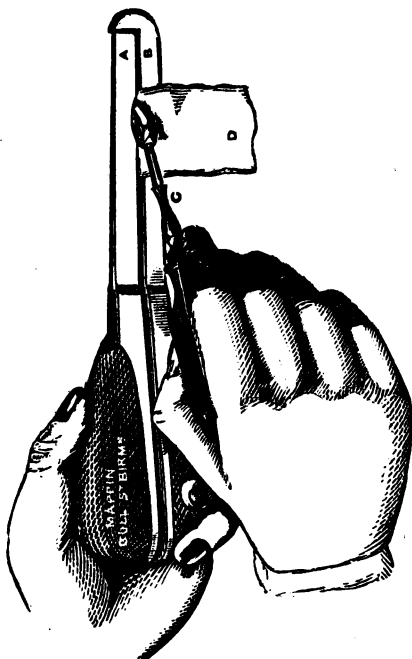
FIG. 1.



Mr. Clay adds that, in a case in which this instrument was used successfully, the number of bleeding vessels of the torn omentum, and the extent of the injury, would have necessitated the use of three ligatures to the omentum *en masse* to have arrested the hæmorrhage.

After the removal of the bleeding portion (nine inches by seven inches), the remaining omentum was returned into the abdomen without further care.

FIG. 2.



A, the male blade; B, the female blade; C, the cautery.

Owing to the compression used, and the application of the actual cautery, the eschar was merely a charred line, so that probably but little irritation was produced by the separation of the eschar.

The instrument is made by Mr. W. Mappin, surgical-instrument maker, Bull-street, Birmingham.

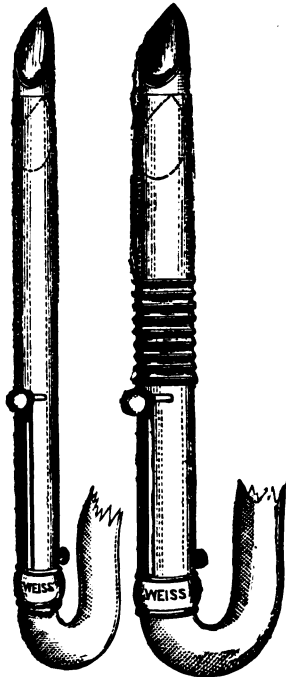
ART. 152.—*A Syphon-Trocar for Ovariotomy and other Purposes.*

By MR. T. SPENCER WELLS.

(*Proceedings of the Royal Medico-Chirurgical Society, July 5, 1862.*)

This instrument consists of a canula, in which, instead of the ordinary solid rod, a hollow tube slides. This tube is sharpened at the end like a tubular needle. The instrument is introduced in the

ordinary manner, and the sharp tube is then withdrawn by the thumb. The fluid flows along the canula to an elastic tube, which may be bent to form the long and short branches of a syphon; and the fluid continues to flow so long as the short branch is immersed, while no air can enter. If suction power be wanted, a syringe may be used instead of the simple tube. The addition of a grooved ferrule outside the canula completes the instrument for ovariectomy.



The cyst, as it is emptied, is tied on the ferrule to prevent the escape of fluid, and to assist in drawing the cyst outwards. If other cysts require tapping, the sharp tube can be immediately projected and withdrawn. The power of doing this with one hand only is a great convenience, not only in performing ovariectomy, but in tapping collections of fluid through the vagina or rectum.

Instruments of various sizes, made by Messrs. Weiss, were placed on the table of the Society.

(c) CONCERNING THE DISEASES OF CHILDREN.

ART. 153.—*On Scarification of the Gums in Teething.*

By Dr. A. JACOBI, Lecturer on Infantile Pathology and Therapeutics at the New York Medical College.

(*American Medical Times*, Sept. 20, 1862.)

Dr. Jacobi holds that dentition is neither a disease nor a direct cause of diseases, except in very rare instances, and therefore he is opposed to scarification of the gums as a rule in dentition. "I see," he says, "very few indications for the lancet during the period of dentition. You may cut where the gums are an impediment to the protrusion of a tooth, or where the gums themselves are the seat of a disease giving rise to general symptoms, especially of the nervous system. Thus, inflammation of the gums justifies an incision, for the sake of relieving the tension of the tissues; the same practice is followed in inflammations of the tongue, of the fingers, &c. Even mild cases in very irritable children may be treated in the same manner. But the incision itself, especially when repeated, may be a cause of irritation, sometimes visible in the fact that during the prevalence of follicular or other stomatitis the gums will be found covered with superficial ulcerations. I need not add, that while exudative processes, such as diphtheria, are active in the system, every wound of this description will give rise to new diphtheritic deposits. I, then, scarify the gum in cases of intense local hyperæmia and inflammation: these are the cases in which the loss of a few drops of blood, which have no effect on either the healthy or the diseased system in general, is decidedly advantageous. I should scarify, and have done so, several times during my practice, in cases of convulsions in tender, delicate, irritable patients, in whom I found the gums swollen, and where a correct diagnosis could not be made instantaneously; especially in such as had been once relieved by the same operation; for I must confess that once or twice in my life, not oftener, I have observed the instant termination of an attack of convulsions after I lanced the gums. But always be sure that the tooth is near the surface. I know that new cicatrices will easily tear, but old ones will not; and I have seen real trouble arising from teeth that had been cut weeks before they were ready to pierce the gums; if you mean to call it a piercing, for under normal circumstances the process is one of slow absorption of the gum. I have known cases in which practitioners had lanced the gums two or three months before the final appearance of the tooth, a practice which is annoying, or useless, or dangerous to the child, and certainly not indicative of much diagnostical power and therapeutical knowledge in the doctor. It is not even uncommon to find a retardation of the protrusion of a tooth where you expected its daily appearance. A child becomes sick, with the symptoms of fever, and some local symptoms which you will or will not diagnose, according to your accomplishments as a diagnostician. You lance the gums, and ex-

pect not only the appearance of the tooth, but also a termination of the untoward symptoms. Nothing of the kind occurs. To the contrary, the child gets thinner and sicker, and no tooth. Where the system is intensely suffering, where emaciation takes place and nutrition is interfered with, it is but natural that the growth of a tooth should also stop. In such cases you may safely predict that no tooth will appear before the child will get well, or at least better. During convalescence the tooth cuts. You say that it made its appearance after the organism had been sufficiently restored to allow of phosphate of lime being spared for the building of teeth; the mother says, that because the child was well when the tooth came and was through, the child suffered from its tooth. You say, the child cut a tooth, after it was well enough. She says, it got well after it cut a tooth. Certainly there are difficulties in teething, but during, not from.

"In one of my first lectures I have spoken of the direct injury done to the tooth by incisions. The consistency of the tooth is the less the younger the child; and the harm done to a tooth by the effect of a hard and sharp instrument cannot be denied. If you expect to effect anything by an incision, you must be sure of dividing it down to the tooth. But you can scarcely avoid injuring the tooth in cutting down upon it. If this danger exists, and it does exist, it is the more to be feared from those often-repeated scarifications recommended by Marshall Hall and others. Thus, while your incisions are of no use in the present, they are positively injurious to the future. There is something absurd and unworthy of the high standing of our profession in performing any, though slight, operation which is useless; but it is a revolting thought to perform one that is worse than useless—viz., injurious. It is unworthy of the high vocation of our profession to resort to an action which gives the impression to the relations of the little sufferer, that not only something has been done, but that the right thing has been done, and which, nevertheless, is destined, in most cases, to cover the want of a diagnosis, and the ignorance regarding the causes of the disease. The language of disease in infantile life is intelligible enough. It is your province to listen to it, and to understand it."

ART. 154.—*On Acute Fatty Degeneration of Newly-born Children.*

By Dr. BUHL.

(*Med. Jahrbücher*, Heft iv., 1862; and *Med.-Chir. Review*, January, 1863.)

The author describes under this term a general disturbance of nutrition, which manifests itself as an extremely acute degeneration of the cell-containing juices of the organs, especially the heart, liver, kidneys, and to a less degree, the lungs and intestines, into fat. It is remarked that most well-nourished children born asphyxiated, pass blood in their stools or in the fluid vomited directly after birth; after from three to six days jaundice and hæmorrhage from the

navel, extravasations of the mucous membrane, of the conjunctiva, mouth, and nose, coming on, as also from the ear, with purpura and anasarca preceding death.

ART. 155.—*On the Connexion between Laryngismus Stridulus and Craniotabes.*

By Dr. A. JACOBI.

(*American Medical Times*, September 13, 1862.)

In a lecture, forming part of a course of lectures on dentition and its derangement, Dr. Jacobi, speaking on this subject, says:—

“There is one disease which appears to be the fundamental cause and origin of laryngismus. It is rhachitis. I merely refer you to a former lecture in order to remind you of these facts, that rhachitis is not only a disease of the osseous tissue, that originally, indeed, it is the result of disorders in digestion and assimilation, and that impaired nutrition brings on anomalies in all the system thus intimately connected with rhachitis. Particularly it is the form of rhachitis which is found in nurslings which is apt to bring on severe and general symptoms, viz., the rhachitical softening of the cranial bones, or craniotabes, of which I have also spoken in a former lecture. Craniotabes is always connected with meningitic processes, effusion between the meninges, and into the brain and its ventricles; and thus its direct connexion with a large amount of cerebral effusion is easily understood.

“Old authors, whose reports Elsaesser has collected in his book on ‘the soft occiput,’ although they did not understand the importance of the rhachitical softening of the parietal and occipital bones, relate a number of post-mortem examinations and cases illustrating the subject. Of the cases of Kopp, one who died at ten months had a very large fontanel, ununited sutures, and very flexible cranial bones; in another who died before the end of the fifth month, he mentions flexible cranial bones, and large fontanels. Caspari relates the case of a child, which was very large and fat, but always had ‘phlegm on his chest,’ and a large head, large fontanels, and swollen epiphyses; he adds, that the majority of his infants affected with laryngismus stridulus had a rhachitical predisposition. Other writers accurately describe cases of craniotabes, the symptomatology of which I have given you in my lecture on the connexion of diseases of the bones with dentition. Thus Pagenstecher speaks of a child who was very large and fat, and was affected with convulsions in his seventh month, and afterwards with attacks of apnœa. Being sick so long, it grew emaciate and thin, and his skull had quite a peculiar, no longer spherical, but remarkably irregular and asymmetrical form. Hirsch found twice, a large head and large fontanels. Keitel describes the attacks, and body of a child who died in his twenty-second week, and had mostly ununited sutures; the small, triangular fontanel remained still open; the quadrangular was unproportionately large, and the skull soft and thin. Hachman has a similar report.

In Günther's child after weaning, 'a true rhachitical constitution' developed itself, and gradually also the attacks of laryngismus. Landsberg also found the sutures open, and delays in protrusion of the teeth. In one case of Hauff's, all the cranial bones appeared of a dark blue colour, and were so little ossified as to be easily cut by means of a knife and scissors, and so thin that the squamous parts of the temporal bones, and some parts of the parietal and occipital bones, had the thickness of good-sized paper. In another, the chest was very similar to the 'chicken chest,' and the commencement of rhachitis could not be denied. A child of Staub's had already in its first year the unmistakable symptoms of rhachitis, and had its first tooth at eighteen months.

"Many such cases could be collected from literature; but those above, taken from older authors, suffice to illustrate the connexion between craniotabes and laryngismus. It is true, however, that not every case of this affection must necessarily be the result of craniotabes. Elsaesser reports the case of a child who had his laryngismus brought on by hooping-cough, not before his craniotabes had healed; and there are a few cases of laryngismus in the second or third years, where craniotabes is generally no longer present. Thus other causes may it bring on; but do not forget, that nervous affections will oftentimes not disappear with the removal of their causes, and that together with craniotabes, alterations take place inside the cranium which are not so liable to heal as the affection of the osseous tissue itself; therefore, craniotabes may still be the cause of laryngismus, even where it appears to have entirely passed by. I hardly remember a case of my own, in which symptoms of general rhachitis and of rhachitical softening of the cranium were absent in laryngismus; thus this much is certain, that the majority of cases of laryngismus, or crowing inspiration, depend on craniotabes and general rhachitis. It is always the great predisposing cause, and thus the last and proximate causes of an attack of our disease, as we find them enumerated in the text books, such as fright, anger, cough, protrusion of a tooth, &c., are thus assigned their right place of but occasional and temporary importance. By the defective condition of the cranium the brain is more subject to external injuries, concussion, quick movements of the head, improper carrying on the arm, lying on a hard pillow, rocking, and high temperature both artificial and solar; and finally, we must not overlook the importance of such alterations as invariably take place, in rhachitis and craniotabes, in the nutrition of the system and the condition of the brain. At all events, you will hardly ever be mistaken in your etiology, when on examining a new case of laryngismus, you look for craniotabes. Whenever a child with laryngismus is brought to me, my first attention is given to the occiput and epiphyses, as my first prescription is almost invariably the regulation of diet and the use of iron."

ART. 156.—*Chronic Induration of the Sterno-Mastoid Muscles in Infants.*

By Dr. WILKS, Assistant-Physician to Guy's Hospital, &c.
(*Lancet*, January 3, 1863.)

Dr. Wilks states that three cases of this remarkable affection have come before him at the infirmary. He believes that the disease must be familiar to the profession, but he is unable to find any reference to it in books, or to glean from those greatly experienced in children's diseases any information respecting its pathology. The publication of these cases may draw forth the experience of others.

CASE 1.—Elizabeth B., aged 7 weeks, was brought to the London Infirmary for Children, Waterloo-road, April 2, 1861, in consequence of the mother having observed a lump on the right side of the neck. On examining the child, a hard, cord-like substance could be felt running in the direction of the sterno-mastoid muscle, its internal edge being very prominent. When the head was bent it was not relaxed, but, being then more easily grasped, it was found to be evidently the muscle which was indurated, for no enlarged glands were present. The child was healthy, and presented no appearance of syphilis. Thinking this might have originated it, some mercury with chalk was ordered, and iodide of potassium ointment. When the period of giving up the latter had expired (at the end of six weeks) the hardness was fast disappearing.

CASE 2.—Jessie D., aged 9 weeks, was brought to the infirmary a few weeks afterwards, the mother having noticed that there had been a hardness on the left side of the neck ever since birth. On examination, there was found to be a hard substance corresponding in position and form to the sterno-mastoid muscle. This had so great a hardness that it felt as if the muscle was changed into tough fibrous tissue, or even into a piece of wood. The child was healthy and well grown, with no history or appearance of syphilis. The previous case having done well, the present was treated in a like manner; and when the child left she was convalescent. It is probable that an equally good result might have come about spontaneously. In this case the mother had observed the induration in the neck immediately after birth, and it must therefore have occurred in utero. It is possible that physician-accoucheurs might show that it had something to do with the position of the fœtus.

The notes of the third case are unfortunately mislaid; but it was that of an infant a few weeks old, who had an exactly similar induration of the sterno-mastoid on the right side. This was fast disappearing when the child was last seen.

ART. 157.—*Two Cases of Infantile Syphilis treated by Chlorate of Potass, without Mercury or Iodine.*

By Dr. DRYSDALE, Physician to the Farringdon Dispensary.
(*Medical Times and Gazette*, and *Dublin Medical Press*, December 3, 1862.)

CASE 1.—G. D., infant, aged 11 weeks, was seen by me at the Farringdon Dispensary, May 17, 1862. This child has now, or has had

for four weeks past, an eruption over the greater part of its body. Has snuffled also during the last fortnight. The eruption is of the papular order, and is copper-coloured; is most profuse over the buttocks. There are fissures around the mouth and anus, and mucous tubercles in the latter situation. Child's aspect is wizened and aged. Mother says that it is wasting away. It suckles freely, but is very fretful, and cries much. Child's father, a bootmaker, is in delicate health, and the mother says he drinks, and is dissipated. The mother has had three children at full time. The first child lived six weeks; the second, two hours; the third, four weeks. The mother is healthy, nor does she seem to have been infected by the children. Child to take, four times a day, a teaspoonful of the following mixture:—Chlorate of potash, ʒj., aquæ, Oj. Great care to be taken that the child be kept scrupulously clean, and the bowels to be kept open by castor-oil. 24th: The papular eruption on the arm is now tubercular in character. Sub-occipital glands much enlarged. Rep. pot. chlor. 31st: Eruption fading. June 18th: Eruption almost gone; sub-occipital glands enlarged. 23rd: Child heavy, almost quite well. No symptoms. October 15th: Met the mother with the child. It is, she says, perfectly well. Has had no return of the eruption.

CASE 2.—An infant, 12 months old, seen at Farringdon Dispensary, May 3, 1862. The child has copper-coloured discoloration in patches over different parts of its body. Left elbow and right knee inflamed and hot. The child has coryza, and is very fretful. The child was born at seven months. The mother suffered from sore throat during pregnancy. Had the snuffles soon after birth. This child was treated by Mr. Allingham with chlorate of potash and hydrochloric acid, and became perfectly well. I should be glad to hear any observations on this subject from persons who have tried both the classical and non-mercurial treatment.

REPORTS
ON THE
PROGRESS OF THE MEDICAL SCIENCES.
January—June, 1863.

THE intention of the following Reports is to pass in review the principal additions to each department of Medical Science which have been placed on record during the preceding six months. It is not contemplated that they should be confined exclusively to the notice of what is new; any fact or doctrine which may be considered practically useful will, although not strictly novel, be regarded as worthy of commemoration. It must be obvious to all who are aware of the immense mass of information which is almost daily put forth by the medical press of this and other countries, that the notice of every subject would be an impossibility. It therefore devolves upon the writers of each Report to select only such articles for retrospection as may possess superior recommendations, either of an intrinsic character, or in relation to the main end and aim of all medical knowledge—the alleviation of suffering and disease.

I.

REPORT ON PRACTICAL MEDICINE.

Studies in Physiology and Medicine.

By the late ROBERT JAMES GRAVES, M.D., F.R.S., &c. &c.

Edited by WILLIAM STOKES, M.D., Regius Professor of Physic
in the University of Dublin.

(8vo. London: Churchill. 1863. pp. 422.)

THIS volume is welcome, as everything must be which helps us to know more of the workings of a mind like that of Graves. The biographical notice which is prefixed by Dr. Stokes, than whom no one could be better qualified to speak of the value of Graves' work, is most interesting; for it exhibits the great physician in a point of view which is new to the majority of the profession, who knew him chiefly if not solely by his writings on Clinical Medicine. A more triumphant refutation of the idea, vaguely existing in many minds, that real progress in medical science is only to be achieved by the men of one idea, who work in one groove, and take little interest in the achievements of the human intellect in fields outside the domain of their own special science, can hardly be imagined. It is plain that just because Graves was an accomplished man at all points—just because his mind was open to perceive the beauty of other kinds of truth besides that which it was the more ostensible object of his life to demonstrate—he was able to rise to that broad and commanding view of medicine in its relations to physiology which placed him far in advance of contemporaries whose diligence and zeal may have fully equalled his own. It is instructive to learn that this great man was a scholar of varied and remarkable acquirements, and an artist with an intense love for and appreciation of the beautiful: and though such a combination of talents and graces in the same individual must be necessarily almost as rare as were the striking personal advantages possessed by Graves, yet we may well indulge in serious reflection on the fact that such were *his* endowments. For in reckoning up the list of distinguished men by whom the great change was wrought which passed over the practice of medicine during the latter days of the last generation, we can see plainly enough that but few, if any, attained a compre-

hensiveness of view like his ; and we are forcibly reminded of what has been said by more than one keen observer, that the greatest genius has always something of universality in its development.

The essays which form the present volume were originally published in various medical journals, or in the Transactions of the Irish Academy. The larger part of them relate to physiological and biological questions, and it is needless to say that in very many instances the views which they contain have been superseded by the more accurate knowledge of the present day. But if any one desires to see how far a vigorous common sense, and a habit of submitting everything at once to a fair comparison with all the known facts, may avail to preserve a philosopher from being seduced by ingenious trains of speculation, we commend to his attention the essay upon Liebig's theories of animal heat and of disease. It is surprising that the pregnant query of Graves as to how the habits of many voracious carnivorous animals and of many equally voracious human flesh-feeders, residing within the tropics, could be accounted for on Liebig's principles, did not at once destroy the prestige of the celebrated theory of animal heat propounded by the latter.

It is almost unnecessary to say that the reader who takes up this volume will be charmed with the purity and grace of style which distinguishes it. There is nothing muddy or confused about Graves' language or thoughts; and to those who expect at any time to have to write on scientific subjects we recommend a study of his works as a model of unpretentious directness and simplicity. But we have said enough of a volume which every one will be sure to read, and which every one will certainly be delighted with.

*Clinical Medicine: Observations recorded at the Bedside,
with Commentaries.*

By W. T. GAIRDNER, M.D., Physician to the Royal Infirmary of Edinburgh, and Lecturer on the Practice of Medicine.

(Post 8vo. Edinburgh: Edmonston and Douglas. 1863.)

This is a very good and interesting book. It is an unfortunate peculiarity of most treatises of clinical medicine that, although they may be recognised as being valuable for reference, scarcely any one reads them through, or indeed takes any very decided interest in them; and thus, for the mass of the profession, even such high-class works as that of Graves remain practically almost sealed books. There are several reasons, however, for which we may venture to hope that a somewhat warmer reception awaits the volume before us. Not only is the author known as a most accomplished physician, but there breathes through every line of what he writes a tone of absolute honesty and fidelity to fact, which is very refreshing in the department of clinical medicine; for it is only too well known that the power of *truthful* description is one of the rarest ever

granted to mortals, more especially to doctors. Another recommendation of the volume is its manageable size and very pleasant readable type; indeed, its excellence in these respects is such as to remove very much of the air of stiffness and dryness which disfigures most works of the kind.

The subjects treated of are just such as would naturally come to occupy the attention of any clinical teacher in a large hospital; and there would be no advantage, even if we had the space to do so, in dwelling on more than a few salient points. In the first place, then, we notice that the author's attention has been strongly directed to the very small degree of mortality which attends the acute inflammations which prevail at the present day, and more especially the pneumonic, pleuro-pneumonic, and broncho-pneumonic affections; that is, at least, when these are pure and uncomplicated. He recognises very clearly the epidemic influences which affect the frequency and severity of these diseases, and even goes so far as to say that the diminution of the latter is as much within the scope of sanitary reform as is that of the so-called "Zymotic" class. With regard to treatment, he exercises a practical eclecticism which acknowledges as its basis the necessity of meeting symptoms as they present themselves, while *au fond* he seems to have a strong and wise reliance on the curative powers of Nature. We notice with respect, though we can hardly agree with them, the author's opinions as to the harmlessness of the antiphlogistic modes of treatment; certainly it is well for us to hear that side of the question fairly stated; and no one can be more entirely trusted to state it without exaggeration than Dr. Gairdner. Still, we are bound to say that we cannot accede to his light estimate of the damage inflicted in a large number of cases by blood-letting, nor can we for a moment admit that the inquiries of Louis and Grisolle "establish beyond all question the power of blood-letting, when employed near the beginning of the disease," to abridge the duration of pneumonia, although we may be quite willing to allow that "in fitting cases" (very rare ones) it may be advisable to employ bleeding as a rough mechanical expedient to save life, or to mitigate very severe pain, even though we inevitably pay a high price for the momentary gain.

With respect to the value of alcoholic stimulants in acute disease we consider the testimony of Dr. Gairdner's book to be particularly valuable. His perfect candour allows the reader to perceive the progressive influence of increasing experience upon his views, and it is quite plain that this progression tends to an increased reliance upon these remedies. At the same time there is no danger whatever that he will advance with rash rapidity in this direction, for he is most careful to disclaim any agreement with the views of Dr. Todd, to which we may say, in passing, that he renders scant justice, and even that he has scarcely understood their drift. We are glad to see that he decidedly objects to the attempts constantly made by the teetotal party to force the medical profession to become the champions of their views, although he falls into the common error of regarding alcohol, even in small doses, merely as tonic medicine, and not at all as food, an error which has received a certain plausible

appearance of support from the very incomplete researches of Lallemand, Duroy, and Perrin. Dr. Gairdner's views with regard to a different sort of action of alcohol upon the organism, the truly poisonous action which culminates, in chronic cases, in delirium tremens, are interesting; and the rules of practice which he lays down are, we believe, sound and just. There can be little doubt that the vast majority of non-traumatic cases of delirium tremens tend to spontaneous recovery if only the nervous system is allowed to recruit itself under the influence of rest and proper nourishment. It is perfectly true that real stimulation of the brain, *i.e.*, bona-fide increase of the activity of the cerebral circulation, would materially assist the influence of mere rest in restoring the nervous equilibrium, and in this way it must be that cures are sometimes effected by alcohol and by opium. But in cases when alcohol has been continuously administered for some time, in poisonous doses, the brain unquestionably loses much of its sensibility to the ordinary stimulating influence of small doses either of alcohol or opium, and if larger ones are given with a view of overcoming the wakefulness, &c., a danger of poisoning is incurred. *Common food*, under these circumstances, forms the true and sufficient stimulant which will raise the cerebral circulation to that point at which sleep is possible; and, consequently, feeding is the true cure for delirium tremens (as it is for acute mania), the patient of course being kept as far as possible in a state of absolute repose. We confess to feeling some regret at the tone adopted by Dr. Gairdner with respect to the so-called "dipsomania." He states his belief in a "moral insanity," of which he gives an example in the case of a particularly degraded drunkard who came under his own care; an example which appears to us to have been clearly one of *general* mental deficiency, or degeneration, or both, and by no means only of "moral paralysis." Extensive observations upon the habits of drunkards have convinced us that such a *special* insanity does not exist, although tendency to drink may and often does show itself as one of the earliest and most prominent symptoms of general mental derangement. With all submission to Dr. Gairdner, we think it would need very much stronger evidence than any which has yet been adduced to establish the existence of such a mental condition as that which he speaks of: and although it is quite true that it would be a public benefit if many persons who are unable to control their appetites were placed under restraint, yet, in our opinion, this benefit would be too dearly purchased if it can only be procured by making a radically vicious distinction between the moral and the intellectual powers as to their liability to "paralysis." We deprecate earnestly the tendency which displays itself in the writings of some of our alienist physicians to erect new and special varieties of insanity, apparently from a mere morbid instinct of compassion towards criminals, and although we are well assured that no such feeling sways Dr. Gairdner, we regret that his influence should favour the purposes of the party from which these ideas originate.

Already we have found ourselves insensibly drawn into the commencement of something like a *seriatim* notice of the individual

chapters of this interesting book, a thing, however, which, as we stated at first, it is out of our power to accomplish. We must hasten, as our space is so limited, to mention, and particularly recommend to the notice of students and practitioners, the chapters which relate to cardiac diseases and intra-thoracic aneurism; these are excellent both as to the value of the facts recorded, and the rules of diagnosis which are laid down. The analysis of cardiac murmurs and their mechanism is most ingenious, and is likely to prove of great service to the student. Among the pathological novelties of the volume may be mentioned some interesting remarks upon the phenomena developed in consequence of pressure (by tumours) on the cervical sympathetic, a subject which will probably receive more careful attention in the future than it has hitherto done.

The book concludes with a chapter on the study of clinical medicine, and some remarks on case-taking, which are models of sensible, kindly talk, such as a professor who wishes to gain the confidence of his pupils should employ. Altogether we may safely pronounce Dr. Gairdner's work to be one of the best adapted to its purpose that has ever appeared, for it unites simplicity and directness of speech with a very thorough and conscientious completeness of all necessary details.

The Renewal of Life: Clinical Lectures illustrative of the Restorative System of Medicine, given at St. Mary's Hospital.

By THOMAS K. CHAMBERS, M.D., Physician to St. Mary's Hospital, &c.

(8vo. London: Churchill. 1863. pp. 426.)

We have seldom felt greater regret than we experienced on the perusal of this volume. Dr. Chambers is known as an able and accomplished physician, and as a bold and independent thinker, and the profession has been made aware from time to time, by means of hints and suggestions, rather than by anything deserving the name of continuous argument, that he holds certain peculiar views as to the nature of disease in general, which, if true, must have very important consequences. What these views are may be shortly stated by a quotation from the first lecture in the present volume:—
 "Disease is in all cases not a *positive existence* but a *negative*; not a new excess of action, but a deficiency; not a *manifestation of life*, but *Partial Death*; and therefore the business of the physician is, directly or indirectly, not to *take away material*, but to *add*; not to diminish function, but to give it play; not to *weaken life*, but to *renew life*."

Now we venture to say that if this theory had been stated as the final conclusion of a series of elaborate clinical researches, and supported by a calm and dispassionate consideration of the existing data of experimental physiology, and if, in addition, it had been

presented in that grave and weighty form which every proposal for radical change in the foundations of practice should be distinguished by, it would not have lacked influential supporters. But the working men of the profession, who have felt a strong interest in hearing all that could be said in support of Dr. Chambers' view, will not feel particularly flattered by the fact that he considers it sufficient to treat them to a mere *rechauffée* of some clever, sketchy clinical lectures, which may have been well enough adapted to their original purposes of arousing the attention of a class of students, and forcing them to think for themselves on the great questions of pathology and treatment. Haste and inaccuracy, such as might well have been excused in oral discourses delivered to such an audience for a particular purpose, mark this volume in many parts; its tone is egotistical also, and indeed is such as to suggest the notion that Dr. Chambers is setting the profession at defiance, and appealing to the judgment or the caprice of the general public. Just to take a single example of the negligent way in which Dr. Chambers examines any theory which does not happen to agree with his own ideas, we may mention that he speaks of the doctrines of Brown, Darwin, and Todd as if they were identical. Now we are not concerned to examine the merits of either of these systems, but we must say that whoever confounds the teaching of Brown with that of Todd must be entirely unacquainted with the *Elements of Medicine*, or similarly ignorant of the principles laid down in the lectures on *Acute Diseases*, if indeed he has studied either of those works.

One word in conclusion as to the external appearance of Dr. Chambers' book. The great potentate of New Burlington-street must surely have been "asleep, or on a journey," when his subordinates designed that cover, and decorated it with that gilt scroll on which we read the "sensation" title, "The Renewal of Life." Not only this, but the very type and paper, and in short the whole get-up of the volume, savours too strongly of a desire to make an impression of cleverness, at a small sacrifice only of money or trouble. We trust, for Dr. Chambers' own credit, and for the credit of the whole profession, that if a second edition of the book be called for, some important alterations will be made, both in the inside and the outside of the volume. At present, we apprehend that so lively an indignation will probably be raised by the flippancy with which so great a subject has been dealt with, that the profession are in danger of overlooking much that is valuable in Dr. Chambers' observations.

On Diseases produced by Morbid Ferments, and on their Treatment. (Sulle Malattie da Fermento Morbifico, e sul loro Trattamento.)

By Dr. POLLI.

(4to. Milan: 1860. pp. 60.)

The conclusion at which Dr. Polli has arrived after an elaborate experimental investigation of the subject, is, that sulphurous acids, and various sulphites and hyposulphites, have the power of arresting all known forms of fermentation, and of retarding putrefaction; and that this power may be made available to the successful treatment of all diseases depending upon fermentative and septic changes, and that, too, without any damage being done to the economy by its exercise. The conclusion involved is nothing less than this—that typhus, nosocomial fever, puerperal fever, dissecting wounds, pyæmia, glanders, cholera, and many other dire maladies, will lose their terrors if properly treated by the administration of sulphite or hyposulphite of soda, magnesia, or some other suitable base; and, most assuredly, there is that in some of Dr. Polli's experiments which makes this hope not altogether Utopian.

It is very evident, in the first place, that the measures proposed are in no sense poisonous. In some of the experiments, large quantities of the sulphites of soda, magnesia, or lime were given to dogs along with their food, without the slightest inconvenience even. In one experiment ten grammes of the hyposulphite of lime were given to a little dog for fifteen days in succession without any appreciable effect upon the appetite or spirits of the animal, and the stomach and intestines at the end of the experiment were found to be in a perfectly healthy state.

It is very evident, also, that the sulphites have a remarkable power in arresting putrefactive changes. In one experiment in proof of this point, three dogs were chosen, all in good health, and all as nearly as possible of the same size and weight, and to two of the three were given in small doses along with their food about fifteen grains of a sulphite—of magnesia to the one, of lime to the other. The third dog had the same food with no sulphite. On the next day, the three dogs were examined, and there was no difficulty in finding traces of the sulphites in the blood, urine, liver, and muscular tissue of the dogs to which these salts had been given. Moreover—and this is the point of interest at present—their fluids and solids were found to be perfectly fresh and sweet five days after the death of the animals which had furnished them, while the corresponding fluids and solids furnished by the dog which had had no sulphite given to it, were fast advancing in the process of putrefaction.

And, lastly, it is evident that these sulphites have even the power of counteracting in a remarkable manner the poisonous workings of putrid blood and pus and of glandered mucus. These matters were

injected into the veins of several dogs, and in some of the experiments the animals were left to their fate, and in others sulphites were administered to them, either by the mouth or by admixture with the matter to be injected into the vein. With one exception, all the dogs died with marked typhoid symptoms in which these injections were practised, without supplying the sulphite as the antidote. Without exception, all the dogs recovered to which the sulphite was administered; and not a few of them were very little affected by the workings of the putrid or poisoned matter.

As yet the clinical evidence in favour of these sulphites in the cases which have been mentioned is very scanty, and therefore it is necessary to suspend our judgment for the present. It is plain, however, that these experiments are very remarkable and encouraging: and it is not less plain that a new chapter in therapeutics is opened to us of which it is difficult to realize the bright contents. If Dr. Polli be right, indeed, it is very plain that we may hope to be able to supply solid help in many miserable cases where at present we are well-nigh altogether helpless. For the rest, we will only say, that from fifteen grains to three drams of sulphite or hyposulphite of soda or magnesia may be given daily in divided doses without any risk or inconvenience.

On the Influence of the Fungi of Musty Straw in the Production of "Camp Measles," and perhaps of Measles generally, and on the Possibility of Preventing Measles by Inoculation with these Fungi.

By Dr. SALISBURY, of Newark, Ohio.

(*American Journal of Medical Sciences*, July and October, 1862.)

Dr. Salisbury supplies us in these memoirs with matter of very great interest. In the first memoir he shows very plainly that a disease like measles may be produced by inhaling and inoculating the fungi of musty wheat straw; in the second memoir he gives twenty-seven additional cases in which this disease was produced by inoculation in the boys of a large school where measles was prevalent, and in which there was some reason to believe that this inoculation had a prophylactic power in protecting the human system from the contagion of true measles. After some general remarks upon the characters of general fungi, Dr. Salisbury relates the following case:—

"Hon. J. Dille, of Newark, Ohio, came to my office on the evening of the 9th of December last, and stated that he was just recovering from what he believed to be an attack of measles. It was his opinion he had caught them from pitching straw from an old stack. He stated that on December 4th he pitched from an old stack a load of straw, and unloaded it in his stable. Portions of the stack had become partially decayed, and were already steaming with the heat of incipient decomposition. In pitching over and picking out the best straw the air became filled with a fine dust, which he freely

inhaled. The dust tasted and had the odour of old straw. This took place during the forenoon. His throat soon began to feel dry and irritated. When he returned to dinner, he could still taste and smell the old straw. This taste and smell he could not get rid of. During the following night he awoke with a very sore throat, which became much worse by morning. After getting up and dressing he was taken with a severe chill, with pains in the head and back, and felt so sick and prostrated that he was compelled to return to bed again, where he remained through the day. The chill was followed by a high fever and severe pains in the head, so much so that a portion of the time he was delirious. He felt a heavy congested feeling about the chest, his throat and fauces were swollen and inflamed, with severe catarrhal symptoms. An eruption like that of measles appeared on his face and neck, and the *old straw* taste still continued. His fever continued high through the following (Thursday) night, with severe pains in the head.

"*Friday, December 6th*, he felt much better, and was able to be up around the house. The fever and catarrhal symptoms had partially subsided. His eyes were sensitive, watery, and inflamed.

"*Saturday, December 7th*, felt much better. The eruption had passed downwards over the whole body, and had begun to disappear from the face. He rapidly recovered, so that on Monday, December 9th, he was moving about the streets. In the evening of the 9th he called at my office. His eyes were still red, inflamed, and sensitive; throat sore, dry, and voice hoarse, and had a heavy congested feeling still about the chest. The blotches on his face could be faintly distinguished. He stated that he could still taste the old straw in his throat."

On the same day (Dec. 4th) that Mr. Dille exposed himself to the straw dust, the measles first made its appearance in the military camp at Newark. It is interesting in that connexion to take into account the following facts:—From the 23rd to the 30th the weather was cool and damp, with considerable rain and snow. On the 1st of December snow fell to the depth of an inch; on the 2nd inst. the weather became quite warm, and the snow melted. Again the bedticks for the soldiers to sleep upon were stuffed with straw and near a fire. Here were present all the conditions requisite for the formation of mould upon the straw, viz., organic decomposition, heat, and moisture, and here were also visible the effects of the exposure of the men to such influences—nor could the disease be traced to any other source. The men came from different parts of the country, and had neither been exposed previous to enlistment, nor afterwards, to the contagious influence of the disease.

On the first day of the appearance of the fever there were eight cases, and within a week after there were forty. The disease then disappeared for ten or twelve days from its first appearance. Between the fourteenth and fifteenth day the disease again appeared, due no doubt to the exposure to contagion. Dr. Salisbury asserts that in almost every instance where camp measles exists the beds of the soldiers will be found filled with straw. As further illustrating the effects of the inhalation of the dust of wheat straw, several farmers

stated at a recent meeting of the "Farmers' Club" in that place, that it was very common after threshing wheat for persons to be taken with severe chills, followed by a high fever, catarrhal symptoms, and an eruption upon the face.

With these observations before him, Dr. S. deemed the subject one for further investigation, and accordingly procured the fungous growths of wheat, and the dust rising from them when agitated, for microscopical examination. The straw used for this purpose was taken from the camp beds, from Mr. Dille's stable, and from stacks in the field. The mould consisted of cells, spores, and sporangia, each element existing in greater or less quantity according to the amount of decomposition which the straw had undergone. He then took clean white straw, free from fungi, packed it firmly in a small wooden box, wet it with a small amount of cold well-water, and placed it with the lid firmly secured near the stove in his office, subjected to a temperature of from 60° to 75° Fahr. Twenty-four hours after, the box was opened, and the centre of the straw was found covered with a mould, and when the mass was agitated a fine dust of spores and cells was disengaged. This, when inhaled, had the odour and taste of old straw. Again the straw was moistened and subjected to the same influences for forty-eight hours, with the effect of increasing the formation of the fungi, and rendering the proportion of sporangia greater. Dr. Salisbury then conceived the idea of inoculating himself, and makes the following statement of its effects:—

"At 10 o'clock, P.M., *Feb. 11th*, 1862, I inoculated my arm with the spores and cells of the fungi of wheat straw, which I obtained by placing a straw—covered with the plants—on a plate of glass and hitting it with a few slight taps. On removing the straw, under and both sides of it was a white cloudy band, about one-third of an inch wide, running across the glass. These spores and cells lay so thick on the glass, that, to the naked eye, they seemed to touch each other. The straw from which I obtained these cells came from a stack near this place, and was the same kind of straw as that used for beds at the camp. Under the microscope the fungi presented the same appearance, and the cells disengaged in agitating the straw were precisely similar.

"*Wednesday, Feb. 12th*.—Perfectly well. No inflammation or itching around the point of inoculation.

"*13th*.—Slight nausea. A very slight redness and itching at inoculating point.

"*14th*.—Got up with a feeling of lassitude and nausea, which continued all day. The redness and itching of inoculating wound increasing; had difficulty in keeping warm; chilly all day; occasional sneezing; eyes sensitive; had a peculiar feeling about the scalp, as if red pepper or mustard had been rubbed into the pores.

"*Saturday, Feb. 15*.—Nausea and lassitude continue; occasional sneezing; flashes of heat over the whole body; itching and inflammation of the wound on the arm increasing; thoughtlessly rubbed off the scab, which was about three lines in diameter. The peculiar smarting, burning, congested sensation over the whole scalp has

increased since yesterday. It extends into the bone, with pains through the forehead and temples. A few blotches have made their appearance on the face and neck. Eyes weak and inflamed, so much so that I could not use them to read over half an hour during the evening. A heavy oppressive feeling about the chest; mucous membrane of fauces and throat dry and irritated; feel as if I had a cold.

"Sunday, Feb. 16.—Had a sensation of weariness and drowsiness, with nausea, all day. Eyes red, inflamed, and sensitive; smart so that I cannot use them to read by gaslight. Whole scalp feels sore, with a constant, congested, burning sensation all through it to the bone. Arm itches; redness as large as a dime. A heavy congested feeling about the chest; have had more or less fever since Saturday morning. Throat and fauces dry and swollen, and voice hoarse. Pains in back and head have been almost constant since Friday last.

"Monday, Feb. 17.—The burning sensation of the scalp still continues. Eyes weak and inflamed; cannot use them long at a time without pain. There is still slight fever and nausea.

"Tuesday, Feb. 18.—Nausea; face feels as if it had been exposed to the heat of an open fire till it had become inflamed. The peculiar burning soreness of the scalp is somewhat relieved. Eyes still sensitive; catarrhal symptoms and fever less than yesterday.

"Wednesday, Feb. 19.—Very much better; the soreness of scalp almost entirely relieved; blotches and redness of face disappeared; catarrhal symptoms and fever gone; eyes quite well."

A second inoculation of himself on the evening of the day of recovery produced no effect save a slight sensitiveness of the eyes. Next his wife was inoculated, and substantially the same constitutional symptoms were produced. He next inoculated a healthy boy, six years of age, who had been exposed to measles by contact with the disease. The fungi, grown in Dr. S.'s "office," were introduced under the skin seventy-two hours after exposure. On the second day, there was redness of the skin about the size of a dime. This was preceded and accompanied by slight catarrhal symptoms. These subsided without any bad effects. In the course of ten days he was perfectly well, and at the time of reporting the case forty-two days had passed, and no signs of measles had yet made their appearance. This procedure was adopted in thirteen similar cases with like results.

The inoculation produces a simple measles blotch around the wound, and is only attended with a slight itching sensation. Dr. S. makes the following remarks in conclusion:—

"I have not been able to distinguish thus far any difference between the eruption and attendant symptoms of genuine measles and 'camp measles,' or straw measles. When the disease is communicated to the human subject, however, by inhaling the spores and cells of straw fungi, the eruption appears to follow the exposure or inhalation in from twenty-four to ninety-six hours; while in exposures to the contagion of the disease, the eruption does not usually make its appearance until from eleven to fourteen days thereafter. It is stated

that in inoculations made by using matter obtained from the measles blotch, or by using the tears, blood, or salivary secretions of subjects broken out with the disease, the modified type of measles which results makes its appearance generally on the sixth or seventh day after the inoculation. In inoculating, however, with the spores and cells of straw fungi, the symptoms commence usually in about twenty-four hours; though sometimes they do not make their appearance till as late as seventy-two hours thereafter.

"This matter, however, requires further investigation before fully reliable statements can be made."

"To what extent inoculation with straw fungi may prove effectual in protecting the human system against the contagion of measles, can only be settled by careful and extended experiments."

The bearing of these facts upon the pathology of hay asthma and of several other affections is obvious, and we are anxious to have further light on the subject. Already, indeed, Dr. Kennedy, of Dublin (*Dublin Quarterly Journal of Med. Science*, Feb. 1863), has supplied a case which tends very much to confirm the conclusions of Dr. Salisbury with respect to the possibility of a disease like measles being produced by the inhalation of the dust of musty straw; and before long, we believe, some observations will be made public which go to show that Dr. Salisbury is not deceiving himself in believing that inoculation with the fungi in question may possibly have that prophylactic power over measles which vaccination has over small-pox.

Cases of Wasting Palsy elucidated by Mr. Lockhart Clarke's Method.

By (1) Dr. GAIRDNER and Mr. LOCKHART CLARKE; and

(2) Dr. RADCLIFFE and Mr. LOCKHART CLARKE.

(1) *Beale's Archives of Medicine*, Oct. 1861.

(2) *British and Foreign Medico-Chirurgical Review*, July, 1862.

The two following cases may be said to inaugurate a new epoch in the clinical investigation of affections of the nervous system—an epoch in which the important researches of Mr. Lockhart Clarke are brought to bear upon the subject. In them, in fact, the microscope may be said to have been used with complete success for the first time. We hope the time is not distant when Mr. Lockhart Clarke will think proper to make his researches public in an accessible form, with a full and clear account of the beautiful method by which he has been able to shed so much light upon the structure of the ganglionic structure of the nervous system: and we think our readers will join in this hope after they have ascertained what has been done by this method in the two following cases.

We leave Mr. Clarke to describe his method of anatomical research as he describes it in the *Philosophical Transactions* (Part I., 1859), and in order that the description may be of real service to

those who are disposed to profit by it, we give it without abridgment.

"The structure or part intended for examination should be as fresh as possible, and cut into portions as small as is compatible with the end in view. These portions I formerly hardened by means of a mixture of one part of spirit of wine and three parts of water, which at the end of twenty-four hours was replaced by a fresh mixture of equal parts of spirit and water, and this again after the same interval was replaced by pure spirit, which ought to be renewed every five or six days. At the end of ten to fourteen days the medulla is sufficiently hard for making sections, which are then subjected to the following process for the purpose of inducing transparency. The sections are first placed in a mixture of one part of strong acetic acid, and four, five, or six of spirit, for a period varying from two or three to ten minutes, according to their thickness. They are then washed in pure spirit, after which, if thin, they are floated on the surface of spirit of turpentine, where they remain until they are quite or nearly transparent, when they are removed to glass slides on which a little Canada balsam has been previously dropped. If now examined under the microscope, they frequently show but little or no traces of cells or fibres—a circumstance which seems to have at first caused Schröder van der Kolk and some others to abandon the method; but if the sections be set aside for some time and treated occasionally with a little turpentine and Canada balsam, the cells and fibres reappear and present a beautiful appearance. Before they are finally covered with thin glass, they should be examined at intervals by the microscope. If the sections be *thick*, I find it best to place them in a shallow vessel, the bottom of which is kept simply wet with turpentine, which can therefore ascend from below while the spirit evaporates from their *upper* surfaces; for the *principle* of the method is this—to replace the spirit by turpentine, and this by Canada balsam without *drying* the sections. The method at first presents some difficulties, and practice is necessary to ensure complete success. Experience, also, will suggest, according to circumstances, many little deviations from the exact rules here given, which to a certain extent must be considered as general.

"For the last three years I have used chromic acid instead of spirit in the process of hardening. This is one of the modifications mentioned in my memoir *On the Medulla Oblongata*.* The medulla of man and the higher mammalia is steeped in a solution of one part of crystallized chromic acid in 200 parts of water, for two or three weeks, and then kept in a solution of about one part of bichromate of potash in 100 or 200 parts of water.† Spirit is used to wet the knife in making the sections, which are first placed in spirit for a few minutes, and then (with or without the previous

* 1857. *Philosophical Transactions* for 1858. Part I.

† For the Rodentia, Birds, Reptiles, and Fishes, it is necessary to use the solution much weaker—about one part to 600 of water, and gradually increase the strength at the end of a week.

use of acetic acid) transferred to the turpentine and Canada balsam, as before.

"Lenhossék, Gerlach, and quite recently Schröder van der Kolk, have adopted this method of rendering sections transparent. Lenhossék uses spirit for hardening the medulla, with some slight modification in other stages of the process. An entire series of very beautiful preparations of the cord and medulla oblongata have been purchased of him by the Royal College of Surgeons of London. Gerlach uses bichromate of potash in the process of hardening; then acetic acid, spirit, and Canada balsam.* He does not mention turpentine; but if this be dispensed with, the Canada balsam must be very thin, and the section must be placed on its surface to allow of the evaporation of the spirit, which will not mix with the balsam, but in contact with it becomes turbid and opaque.

"In his essay on the Spinal Cord published in 1854, Schröder van der Kolk says that my (second) method, 'which appears to have been extremely successful,' did not succeed with him; 'the preparation became only partially clear, and the ganglionic cells, from which the fat seemed to be removed, were rendered indistinct.'† It is perfectly clear that he had not sufficient practice in the process. He says again, in speaking of Lenhossék, 'he follows Clarke's method with turpentine, which, according to my experience, cannot lead to correct results, as the spinal marrow is too much altered by that agent.'‡ After criticising every other method, he describes his own, which consists in the use of concentrated chloride of calcium, and which, according to his experience, 'deserves to be preferred to all the others.' But now we find that he has abandoned it, and adopted my method in conjunction with the colouring process recommended by Gerlach; for in a supplementary paragraph to the English translation which has just been issued by the Sydenham Society (1859), he states that after some trials he finds this mode of making preparations the most satisfactory, and in many respects still better than with *chloride of calcium*. The section, after having been coloured with solution of carmine and washed in spirit, is laid in a watch-glass and allowed to dry a little, after which, 'a couple of drops of spirit of turpentine are added. If it is now left for twenty-four or forty-eight hours without a covering glass, but protected from the dust, the spirit evaporates, and the object becomes perfectly clear. It is now covered with Canada balsam and a glass.'§ The colouring with carmine is often very useful, but I think it rather interferes with the sharpness of the fibres. I have long used, occasionally, a colouring fluid known to painters and wood-stainers by the name of *archel*. When carmine is used, the solution should be previously filtered, otherwise a deposit or crust is apt to form on the object; and for the same

* *Mikroskopische Studien*, p. 2. 1858. Erlangen.

† *Translation of Sydenham Society*, p. 30, 1859.

‡ *Ibid.*, p. 28.

§ *Loc. cit.*, p. 33.

reason, the section, after having been coloured, should be washed in water before it is placed in the spirit, which readily precipitates the carmine.

"Stilling has given a full, but somewhat inconsistent and incorrect criticism on my method. He concludes by saying, 'While it is allowed that by the employment of Clarke's method many truths indeed in reference to the minute structure of the spinal cord may be brought to light, and that Clarke's labours must be said to be quite trustworthy (recht anerkennenswerthe), it must nevertheless be observed that the condition of certain textures, such as the elementary structure of the primitive nerve-fibres and the nerve-cells are so altered by it, that this method in many respects must be considered as an obstacle to the more exact kind of investigation.'*

"Now, although I am not so blindly prejudiced as to maintain that preparations made by this method are in every respect perfect, I do maintain that it produces but little alteration from the natural appearance of the nerve-cells, and that almost the only structure that appears to suffer more than from chromic acid alone is the white substance of the nerve-fibre; and even this may be perfectly preserved if the medulla is placed first in a weak solution of chromic acid and then in a solution which is much stronger. But even the *loss of the white substance* would be of no consequence for tracing the *course of the nerve-fibres*, the axis-cylinders of which in these preparations are rendered unusually strong and distinct. And if I wish to investigate the *natural structure of the nerve-fibres and cells*, I select a perfectly fresh and unaltered specimen from an animal just killed (as I stated in my first communication to the Royal Society, 1851); for I never think of trusting to any kind of preparation, or even to chromic acid, which Stilling uses, and which coagulates the contents of the nerve-fibres and alters their appearance after a short maceration. Not that I reject other methods: for I find sections of the medulla, simply hardened in chromic acid, of great service, and use them largely, particularly for drawing figures under low powers, and sometimes for examination under higher powers, when I have been in some doubt as to the nature of the tissue. In fact, I employ whatever means appear most suitable for the occasion and most likely to lead me to the truth. But I still contend, that for clearness, sharpness of outline, and fine definition under the higher powers—for the advantage of obtaining *thick sections of great transparency*, and for the durability and unchangeableness of the preparations when properly made, I know of no method that will bear any comparison with this. To confirm this opinion in one respect, I may mention the fact, that although Stilling, in his recent work, has described and represented the spiral structure of the fibres of the white columns of the *Calx* under a power of 1100 diameters, he has failed to detect the nucleated cells and nuclei which are so beautifully seen by means of my method under a power of 350 diameters."

* *Neue Untersuch.*, 5 Lief, p. 1071.

The two cases which follow are given at unusual length, but not at greater length than their importance deserves.

CASE I.

Part 1. By Dr. Gairdner.

Dr. P., æt. 65, of sanguine temperament and full habit, engaged in literary occupations, in the end of 1855 began to complain of neuralgic pains in the balls of the thumbs of both hands, which before long extended to the forearms and arms. Marked weakness of the hands, more especially of the right, gradually manifested itself, and was followed by diminution in size of the muscles of the thumbs and index fingers, which also became permanently bent inwards and towards the palms. In the summer of 1857, there was manifest deformity of the hands, which became flexed towards the wrists, with a constant tendency to assume the prone position. The position of the hands and fingers at once conveyed to medical observers the impression of the "dropt wrist," resulting from lead poisoning. There was, however, an entire absence of cachectic appearance; there had never been colic; the bowels were not markedly constipated; there was no blue line on the gums; and no trace of lead could be detected in the urine after a most minute chemical examination.

The patient, when first seen by Dr. Gairdner in October 1856, further complained of some degree of difficulty in walking, as well as of pain in the lower limbs, which had been a very early symptom, and also of a liability to trip in going down a stair. There was, however, no distinct indication of paraplegia, either in his gait or in his power of retaining his balance in the upright posture, although latterly the patient became very inactive and unwilling to walk.

The advance of the disease was attended by an almost childish degree of helplessness, and a pitiable state of mental irritability and hypochondriac depression, while up to a late period his appetite continued good, his colour florid, the tendency to take on fat was not checked, and the pain never appeared to be acute. He constantly complained of the "torture and agony" he experienced, and displayed a morbid craving for sympathy. Still, with great loss of self-control and mental energy, there was no delirium, no disorder of the senses, no inconsequence of reasoning, and no delusion; his sight was good for his years; his hearing was unimpaired; his articulation was perfect; his memory seemed to be good; and, except that his constant preoccupation with his sufferings prevented the right use of his mental powers, his intellectual and moral faculties were wonderfully little affected by the disease. There was never any headache, dizziness, numbness, or impaired sensibility; no disorder of the special senses; never anything approaching to coma or convulsions (with a doubtful exception in November, 1856); there was no paralysis of the articulation, of the voice, or of the respiration; and, in general, the voluntary movements, with the exception of those of the specially affected groups of muscles, were normal. From a very early period in the history of the case there was a very distinct difference in the size of the pupils,—the right being constantly larger, by perhaps one-third, than the left. Neither pupil, however, could be said to be decidedly beyond the range of physiological difference; and it was very difficult to determine anything positively abnormal in the movements of either, considered apart from the other. There was no corresponding difference in the distinctness of vision in the two eyes.

Within the last year or two of Dr. P.'s life there was a gradual falling off in strength and general health, with an equally gradual and regular increase

in the rapidity of his pulse, which, from 65 at the beginning of his illness, had risen to 100 and upwards towards the end of his last year. The respiration was not proportionately affected. He could not be prevailed upon to take either walking or carriage exercise, and for many months was unable to feed himself or use his hands in any serviceable way. His death occurred in January, 1861, and was preceded for a few days, or probably caused, by an attack of laryngitis.

The post-mortem examination was most carefully performed by Mr. Bayldon, in the presence of Drs. Gairdner, Adamson, and Bell, two days and a half after death. The general result, as regarded the organs of the thorax and abdomen, was that there was no appreciable amount of disease. The *larynx* alone presented anything which could be supposed to account for the difficulty of respiration experienced in the last few days of life. The appearances there were those of distinct, though very slight, oedema glottidis, with considerable congestion of the mucous membrane extending towards the trachea.

The *arteries* generally were slightly atheromatous, and there was a considerable deposit of fat in the thoracic and abdominal parietes, as well as in the mesentery and omentum.

The *brain* appeared strictly normal as regarded both the grey and white matter. The *arteries* at the base were considerably atheromatous, and there was a slight degree of opacity of the arachnoid from the olfactory nerves backwards to the commencement of the medulla oblongata, as well as in the fissure of Sylvius, and about the inferior vermiciform process of the cerebellum.

The *nerves* at the base of the brain were natural, except that the right third nerve was distinctly smaller and duller in colour than the corresponding left nerve.

The *cerebellum*, *pons Varolii*, and *spinal cord* were placed in a solution of chromic acid, with a view to their being transmitted to Mr. Lockhart Clarke.

The *spinal cord* was unfortunately injured at one point while being removed.

The *nerves* arising from the medulla oblongata and spinal cord were generally examined as to their size and other characters, but without any positive result.

The balls of the thumbs were much atrophied, the flexor muscles of the thumb pale. The muscles of the forearm generally were small and rather flaccid, but their colour was well preserved. There was no very apparent difference between the flexors and extensors.

The atrophied muscles were examined microscopically, and proved to be at some points almost devoid of striæ; but although very slightly granular in the ultimate fibre, they were by no means in an advanced state of fatty degeneration.

Part 2. By Mr. Lockhart Clarke.

In the cervical enlargement of the spinal cord there was a deep, broad, ragged wound, which had entirely destroyed the posterior white columns, with the whole of the posterior cornua or grey matter, as far forward as the central canal. The morbid change which existed at the part had probably made it more than usually liable to damage.

Internal state of the cord.—With the exception of a considerable deposit of *corpora amylacea*, there was no actual change of structure through the whole of the lumbar and dorsal region, to the lower end of the cervical enlargement. In the cervical region, however, there were decided evidences of morbid changes of structure in the posterior grey substance, extending in a variable degree from the lower end of the cervical enlargement to the third

cervical nerves. The lesions were probably more extensive than elsewhere at the middle third of the cervical enlargement, which was accidentally destroyed. When the cord had been hardened by chromic acid, and thin sections of this portion were subjected to a magnifying power, the posterior grey substance, particularly on the right side, was seen to be interspersed with a number of unnaturally transparent streaks, patches, or spots, of different shapes and sizes, these spots being most frequently found around or at the side of bloodvessels. In some sections these morbid spaces appeared as mere fissures or cracks, which, under a low power, might have been considered as the result of accident, had they not been so uniformly found in only one portion of the grey substance, and more on one side than on the other. But when a sufficiently high power was employed, it became evident that these were not merely vacant spaces, but composed of a substance which differed entirely in its nature from that of the surrounding tissue. This substance had a delicate, transparent, and very finely granular aspect. The granules were more closely aggregated towards the centre of the mass, but were generally so fine that they could not be distinctly seen under a power magnifying much less than 400 diameters. There were no traces of granular corpuscles. Sometimes at the edges of these morbid spaces there seemed to be a kind of transition or degeneration of the surrounding nerve-tissue into the granular substance of which they were composed. In some instances the broken ends of nerve-fibres proceeding from the posterior roots were seen to project into opposite sides of these spaces, across which there was strong reason to believe that they had once been continuous. In ascending the cord, from the upper third of the cervical enlargement, the morbid appearances diminished in extent, and gradually disappeared about the third pair of cervical nerves. The *corpora amylacea* stated to have been met with in the dorsal and lumbar portions were found in still greater numbers in the cervical region of the cord; they varied in size from that of a blood disc to the 1400th of an inch in diameter, were thickly accumulated round the central canal, and extended in small numbers through the whole of both the anterior and posterior commissures, but not beyond them.

The *medulla oblongata* had suffered no actual lesion either in its grey or white substance, but throughout its entire length was a large accumulation of *corpora amylacea*.

The whole floor of the fourth ventricle, instead of being smooth and shiny as in the healthy state, was paved with a multitude of granulations or small rounded eminences, very closely aggregated together. They adhered with some tenacity, and when examined microscopically were found to consist of globular aggregations of the ordinary epithelial cells, which in the healthy state are arranged side by side, and form a smooth surface on the floor of the ventricle. The central part of the medulla beneath the fourth ventricle was in a softened state; but this appeared to be a post-mortem change, as there was an entire absence of granular corpuscles, and no indication whatever that the softened condition was the result of inflammatory action.

CASE II.

Part 1. By Dr. Radcliffe.

Mr. Frederick P., aged 40, a native of the United States of America, and formerly a surgeon in the U. S. army, was admitted into the Westminster Hospital, under the care of Dr. Radcliffe, on the 17th April, 1861.

Present state.—Mr. P. is in bed, propped up in a semi-recumbent position by pillows. His countenance is bright and intelligent, his complexion

remarkably pale and transparent, his body and limbs greatly emaciated, especially the arms, which are literally little more than skin and bones.

Asking him whether he was able to change his position, he gave utterance to some low, unintelligible sounds, and moved his legs about, but not his arms. On further inquiry, the arms were found to be wasted to the last degree, stretched out towards the pubes, and somewhat pronated and flexed, the relics of the muscles being tense and rigid, and altogether disobedient to the will. The legs could be moved about in any direction without much difficulty, but somewhat slowly, and all power of standing or walking was absent. In the arms no sign of contraction could be produced by percussion or by the shocks of an induction coil, beyond certain slight flickerings in parts of the deltoid and great pectoral muscles; in the legs, no reflex movements could be produced by tickling the soles or calves. Examined carefully by the points of a pair of compasses as well as by pinching, there was no appreciable change in common sensation anywhere, not even in the hands and arms. Very expressive changes in the countenance, accompanied by shakes of the head, and certain faint inarticulate sounds, showed very clearly that pinching and tickling were by no means agreeable to the patient—showed, in fact, that the blight which had abolished speech and palsied the body to a great extent, had not extended to the mind. And Mrs. P., who was standing by the bedside at the time, removed all doubt upon this point, by saying that her husband was “too intelligent, if anything,” and that he was never tired of hearing read books requiring attention and thought. “I only wish he would sleep more,” she said, adding as her opinion, that sleeplessness and occasional attacks of difficulty of breathing were much more prominent and distressing symptoms than the palsy.

On asking to see the tongue, this organ was found to be wasted, flaccid, and curiously slow in its movements. It did not appear to be more than half the usual size, and it certainly required many efforts before it could be got beyond the teeth. There was no deflection or protrusion. Deglutition was slow, and only accomplished with much difficulty. The appetite was pretty fair; the bowels somewhat constipated, but perfectly under the influence of the will. Nor was there anything wrong in relation to urination.

All the time of the examination, the breathing was disturbed and hurried, more so at the beginning than afterwards. At first the walls of the chest were almost motionless. Now and then, after every twelfth breath or so, there was a pause, followed by a deep-drawn sigh. The pulse was about ninety, and not particularly wanting in power.

In other respects, there was nothing positive requiring notice. There was no pain anywhere, and no tenderness in the cervical or any other region of the spine. Nor was there anything faulty in the action of the special senses.

Previous History.—Mrs. P.’s account is this—Mr. P. now and then giving assent by nods :—Two years ago, while serving in California, he had a sunstroke, which made him insensible for half-an-hour, and left him weak and shaken for several days, but not to such a degree as to prevent him from resuming his duties within a week. A month later, finding his hands and arms becoming very weak, he was obliged to give up his appointment. Soon after this he was stunned by some ruffians, and robbed of all he possessed. Later still, after an interval of a couple of months probably, he got a place as surgeon on board a small vessel bound for England, and in this way he reached this country thirteen months ago. He had been incessantly sick all the way, and when he arrived he was emaciated to the last degree, and scarcely able to move hand or foot. A week or two later, he

was admitted into Guy's Hospital, and there he remained until a few days ago. He ascribes his exceeding paleness to a course of mercury, pushed to salivation, and continued over several weeks after his arrival in this country. Prior to the sunstroke in California, he had never, so he says, had a day's illness, but he allows that he had led a very dissipated and intemperate life up to that time. . . .

April 18th.—Mrs. P. says her husband has had a much better night than usual, and less difficulty of breathing upon awakening, and he assents by nods. He is breathing more freely than he did yesterday, there being now evident movement in the sides of the chest, though by no means as much as there ought to be. Upon carefully examining the chest by percussion and auscultation, there was found to be nothing in the state of the lungs or pleuræ to account for the hampered state of the breathing. At the time of this visit, he was trying to swallow a morsel of bread sopped in beef-tea, and he must have been at least five minutes before he succeeded. He likes the galvanism, and thinks it does him good.

April 20th.—In very good spirits, thinking himself decidedly better.

April 22nd.—Dead.

Death happened suddenly in the night. At 4.30 A.M., the night-nurse left him awake, and as well as usual; a few minutes afterwards she was summoned from one of the adjacent wards by one of the patients, and before she could reach him all was over. The patient who summoned the nurse, and who slept in the next bed, said that he was awakened by the sounds of struggling and difficult breathing, and that he at once jumped out of bed and ran to find the nurse. On the previous day, he had expressed himself in some way intelligible to his wife as certain of recovery, and at bed-time he was quite quiet and comfortable.

Post-mortem examination twelve hours after death.—Body exceedingly emaciated, especially in the arms, which, as before said, are literally little more than skin and bones. Rigor mortis universal and perfect. No trace of injuries about the head or elsewhere. The brain and its membranes perfectly healthy.* The membranes of the spinal cord healthy, but the cord itself without any evident brachial enlargement. Looking at the cord as it lay in its canal, it seemed to be of the same diameter throughout; it seemed, too, as if the nerves proceeding from the brachial enlargement were smaller than they ought to be. No sections were made, as the whole cerebro-spinal axis was reserved for special examination hereafter. The thoracic organs presented no sign of disease beyond these, that the heart was a little paler and smaller than usual, and that the lungs were somewhat gorged with blood. The abdominal viscera were all healthy.

From a statement furnished by Dr. Gull, regarding Mr. P.'s condition when he came under his care in February 1861, it appears that "at this time the muscles of the upper extremities were thin and wasted to the last degree. There were still traces of contractility of the muscles under galvanism, most marked in the right arm. The sensibility was diminished, but not lost. The muscles of the lower extremities were thin and feeble, but not paralyzed. He could flex the legs, and the sensation was not greatly lessened. He retained power over the sphincter. The urine was acid and normal in colour. The articulation was much affected, so that he could hardly make himself understood. The tongue could be put out, but

* "This is evidently a mistake in the notes; for, on examining the brain, Mr. Clarke found the membranes firmly adherent along part of the marginal convolution of the longitudinal fissure, and the cortical grey substance corresponding thereto in a softened state."

only to a slight extent, and with a tremulous quivering motion. The eye was intelligent, and the features not inexpressive. Deglutition was impaired.

Part 2. By Mr. Lockhart Clarke.

In the *lumbar* region, the diameter of the cord was not appreciably diminished; but in transverse sections, it was evident that through the *middle* of the enlargement the anterior cornu of grey matter was rather smaller than natural. The nerve-cells composing the large groups were found to be much reduced in number; but, on very careful examination of sections, prepared according to Mr. Clarke's own method, the missing cells could be detected lying between the others, much diminished in size, none being larger than the *nuclei* of the surrounding healthy cells, without any trace of nuclei or distinct granular contents, and in some cases resembling shrivelled sheaths, or radiating portions of connective tissue. The central canal was about the normal size, but the epithelium around it was increased or hypertrophied. In the upper third of the lumbar enlargement, the anterior cornua were but little affected, but there was the same hypertrophy of epithelium around the canal; and beyond this, chiefly in the substance of the posterior commissure, there were in some sections small irregular or oval patches, of a clear or finely granular material, which would seem to have been originally semi-fluid. Scarcely any traces of corpora amylacea were observed; but both here and in the anterior grey substance the bloodvessels appeared larger than usual. The posterior cornua were in every respect healthy.

In the *dorsal* region of the cord, not only was the grey matter diseased, but there was considerable displacement of some of its parts. In most cases the posterior vesicular columns were normal in size and structure, but in some sections the left column was the smaller, and in many both columns were displayed unsymmetrically with regard to the other. In almost all these sections some of the nerve-fibres of the transverse commissure, between the vesicular columns, were more or less injured, while some of them were entirely lost or eroded. In this commissure, and in other parts around the canal, were some small round or oval spaces, filled within with a pellucid or more or less granular material. They were most numerous towards the middle of the dorsal region, and were most frequent around bloodvessels. In the upper part of the dorsal region, the cord was least affected; indeed, there was scarcely any morbid appearance, save a trifling hypertrophy of the epithelium and connective tissue around the canal.

In the *cervical* portion of the cord there was extensive disease. That portion of it known as the brachial or cervical enlargement was not larger in diameter than any part of the same region, so that in reality there was no cervical enlargement at all. On making transverse sections, it was perceived that the anterior cornua of the grey substance were unnaturally small, and somewhat altered in shape. At first, scarcely a vestige could be seen of the large groups of cells which are found in corresponding parts of the healthy cord; but when the sections were prepared according to Mr. Clarke's own method, the entire groups of cells could be distinguished, although the cells were wonderfully altered in appearance, all being more or less atrophied or shrivelled. In the upper part of the brachial region, the number of healthy cells was greater than in the middle and lower part, but their proportion to the atrophied cells was still small. Through the remaining cervical portion of the cord there was a similar state of atrophy of the grey substance.

All the white columns of the cord in every region, but particularly in the

cervical region, had suffered more or less from atrophy or degeneration. In the nerve-fibres the change was observed chiefly in the axis-cylinders, which were frequently reduced to less than one half their normal diameters. Between the fibres, however, the connective tissue was unnaturally abundant; and the hypertrophy of this tissue, with atrophy of the white nerve-substance, appeared to be the chief causes of the displacement of parts already described. In the cervical region, the anterior roots of the nerves were decidedly below their average size.

No part of the *medulla oblongata* was perfectly healthy. At its lower part, the only morbid appearances were occasional and trifling atrophy of the nerve-cells in the anterior grey substance, with some hypertrophy of the connective tissue of the *white* substance. But from about the lower end of the olivary bodies to the commencement of the fourth ventricle, the morbid changes were much greater and more extensive. This space includes the principal part of the grey tracts or vesicular centres, from which the hypoglossal or lingual nerves take their origin. Above the calamus scriptorius, these tracts or cylindrical columns of nerve-cells form part of the floor of the fourth ventricle along the sides of the median line; and below the calamus, they lie in front of the central canal, at the sides of the median raphe. They consist of large multipolar nerve-cells, like those of the anterior cornua of the cord, of which they are the analogues. Now these grey columns were, in some parts of their course, reduced to about two-thirds, and, in other parts, to one-half their natural diameters; while their cells, like those of the anterior cornua of the cord, were more or less shrunk or atrophied. The hypoglossal or lingual nerve-roots, in their course through the medulla, were also in some places not more than half their natural size, and in other places could scarcely be discerned.

The central nucleus of nerve-cells which gives origin to the *upper* roots of the spinal accessory nerve, was not appreciably affected.

Both of the olivary bodies, with their transverse decussating commissure, were healthy; none of their numerous cells appeared to have suffered any degree of atrophy; but behind them, in the central parts of the medulla, some of the nerve-cells scattered amongst the transverse arciform plexus, as well as the axis-cylinders of longitudinal fibres, were affected in this way.

The fourth ventricle, pons Varolii, and indeed every other part of the encephalon, except one of the superficial convolutions, were in a healthy state. This was the marginal convolution of the longitudinal fissure (*Première convolution de deuxième ordre*, of Foville). For about an inch of its length, near the middle of its course, the membranes were very closely adherent to its grey surface, which was softer than natural, and could not be separated from them without laceration.

On Diseases of the Supra-renal Capsules, or Morbus Addisonii.

By Dr. SAMUEL WILKS, Assistant-Physician to Guy's Hospital, &c.

(*Guy's Hospital Reports*, 3rd Series, Vol. VIII. 1862.)

Since the publication of Addison's treatise on diseases of the supra-renal capsules there has been a growing feeling of scepticism as to the reality of the disease therein described. It was evident that the author had not made out his case fully in this treatise, and yet there were many reasons for believing that there was a case to be made

out. It was evident that a revised edition of the original memoir was wanted, in which the chaff was winnowed away from the grain, and new grain added to the old; and it appears, from what Dr. Wilks says, that Dr. Addison was fully alive to this necessity.

Now, what Dr. Addison did not live to do, Dr. Wilks has done; and what Dr. Wilks has done, no one else was so well fitted to do. In every respect, indeed, is Dr. Wilks fitted for the work he has undertaken. He is well acquainted with all the cases described by Dr. Addison; he examined all these cases pathologically; he has carried on the inquiry since Dr. Addison's death with the greatest diligence; and besides all this, he has, as is well-known, all the collateral knowledge which is necessary to give weight to his opinions. All this is proved by the contents of the memoir under consideration—a memoir which may be considered as a second edition of Dr. Addison's monograph, with the addition of no less than twenty-five new cases.

What, then, is the result of these new inquiries in this interesting subject? Does it strengthen or weaken the original argument? Upon this point Dr. Wilks speaks plainly. He says:—"I had no doubts whilst Addison was preparing his work, and I certainly have none now, after having witnessed or had knowledge of tenfold the number of instances which he himself brought before the profession." And in another place he speaks of his own cases as substantiating, "beyond disputation," the original facts of Dr. Addison. This is saying a great deal, but it is not saying more than what appears to be strictly justifiable by the evidence. Indeed, after going carefully through this evidence, we are constrained to adopt Dr. Wilks' conclusion without reservation.

The important features of the disease under consideration, as sketched by Dr. Addison himself, are these:—a remarkable progressive feebleness of the patient without any apparent known cause, or as it is styled, *asthenia*; a discoloration of the skin; and a disease of the supra-renal capsules. "The principal exceptions or modifications to be made in reference to these three great truths," says Dr. Wilks, "are that discoloration may occasionally be absent, and that when present it is characterized by a general change of hue over the whole body, and also that it is not unimportant as to the nature of the morbid material in the capsule, but that the change in the organs is of a peculiar character. If, then, these propositions be so modified, it may be stated very positively that when these two former conditions—the *asthenia* and discoloration—have existed for any period, that the third, or disease of the capsules, may be very safely predicated."

With respect to the discoloration or pigmentation of the skin, Dr. Wilks says:—

"This is certainly one of the most remarkable changes which is known to occur in the course of any disease, and is that especially by which Addison's disease has been at first suspected to exist by those who have only casually heard of his researches. It is no wonder, therefore, that so prominent and marked a condition should have led to many mistakes, and that a brown skin should have been

supposed to have necessarily denoted a disease of the capsules. Such an imperfect view of the nature of the disease has led to numberless errors, and caused much doubt to be thrown upon the correctness of the original views. Having already alluded to this subject,* I shall repeat mainly what I said on that occasion.

"The discoloration of the skin, although a striking feature of the complaint, was not the main one insisted on by Addison, but since it is that which can be portrayed in a drawing, and, consequently apt to strike the eye on turning over the pages of his monograph, it is not surprising that it was at once regarded as the most remarkable part of the complaint, and would, therefore, be especially dwelt upon in our ordinary mode of communicating to one another the facts relating to the disease, until at last the erroneous opinion would be reached that Addison's disease and discoloration of the skin were convertible terms. Now, it is stated by the author himself that he was led to the discovery by a very different method than by studying the changes in the skin—that it was owing to a peculiar interest which he took in a class of cases styled simple and fatal anæmia that this allied affection came under notice, and that it was whilst watching such cases the fact of the discoloration was also observed, and, subsequently, on post-mortem examination, its connexion with disease of the supra-renal capsules. The extreme prostration of all muscular power was the most remarkable circumstance attending these cases; and, indeed, in some forms of supra-renal affection no discoloration has occurred at all. This was the case in the instance of a man whose case is described as No. 8, in whom there was no discoloration of the skin, the only symptom being the most utter prostration of strength, and yet after death these organs were found completely diseased. The most prominent symptoms, then, of the affection are due to this asthenic condition, denoted by the loss of muscular power, weakness of pulse, breathlessness upon exertion, dimness of sight, weakness of stomach, &c.; and if the case has been of long duration, in addition to these, a discoloration of the skin. A sufficient number of cases have now been observed to suggest whether the change in the skin does not depend on the chronicity of the disease, and that if it should progress rapidly no discoloration would be observed, the symptoms being merely those of asthenia.

"As regards the character of the colour when it occurs, the observation of several instances, since the publication of the original memoir, has shown that a great similarity has existed in all of them, both as regards the hue itself and its method of affecting the body by a uniform implication of the whole surface. It is true that a case is represented where the body is covered with patches of colour, checked with white; but here no post-mortem examination took place to verify the diagnosis, and thus the fact is left as we state, that in the first related cases, as well as in all subsequent ones, there has existed a uniform discoloration of the whole integument. So remarkable has this been, that the body has presented the appear-

* *Guy's Hospital Reports*, Series iii., vol. v. p. 93.

ance of a person with dark blood, rather than that of a European. Wherefore the answer which Dr. Addison was continually making to interrogatories respecting the peculiarities of the discoloration was, that he regarded only those cases as characteristic, where the surface of the body was seen to be gradually approaching in colour that of an inhabitant of some southern nation. The similarity is proved by examination of the integument itself, which, if placed beneath the microscope, is seen to contain a layer of pigment in the rete mucosum, just as in the dark races of mankind; so that no difference, as far as we are aware, can be discovered between them. Moreover, in the latter, certain parts, as the axillæ and pubes, are darker than the rest of them; so in this morbid discoloration these same parts are those most affected, the only difference being that the parts most exposed are those first and most affected, as the face and hands. Also those parts where a blister or an eruption may have excoriated the surface; and in one case (No. 21) it will be seen that there was a darker mark round the leg where the garter had been tied. The effect of the sun is most important to remember, as it may be a source of fallacy, by creating a suspicion as to the presence of supra-renal disease, just as it may divert attention from the true nature of the disease, when the supra-renal affection is actually present. This occurred in two of our cases. It must also be noticed that those parts where most pigment naturally exists become darker than other parts of the skin.

“Without denying that the colour may sometimes occur in patches, we think we are correct in saying that all experience has hitherto shown that the discoloration has been uniform over the whole surface of the body. The exact hue is difficult to describe, but it may be said to resemble that of a mulatto's skin, and therefore is of a brownish cast, having sometimes an olive-green tinge, and thus the term bronzing of the skin has come into use; or it has often what our artist calls a walnut-juice shade. To say, however, in short, that the appearance produced is exactly that of a person of dark blood, is to speak as accurately as description will allow. The anatomical characters are probably not very peculiar, and resemble those of many other pigmental changes arising from various causes, since the true seat of the colouring matter is in the cells of the rete mucosum, as described in the following cases. This is also its position in the dark races. An examination of a portion of the skin, therefore, would not determine the nature of the case; although, if the colour should be universal, there would be a strong suspicion of the existence of supra-renal affection; that is, I do not know of any other disease which changes a white man into the appearance of a black one. It is well to remember that the pigment is seated beneath the epidermis, and thus is to be distinguished from pityriasis and many other conditions with which Addison's disease has been confounded. It would seem scarcely needful to say that it is impossible to confound it with jaundice, had the mistake not often been made; indeed, in nearly all instances of the affection which have come before me, an hepatic disease has been suspected; but a moment's observation would be sufficient to show that no bile is

circulating through the body—the urine contains none; nor is the conjunctiva yellow, which is the first part affected in jaundice, but here, on the contrary, is remarkably pale and anæmic. It is true, however, that in jaundice the yellowness of the skin is due to a deposition of biliary pigment in the rete mucosum.”

As to the actual nature of the disease in the capsules, Dr. Wilks says:—

“I have already said that nothing is known of any other than one disease which produces the remarkable symptoms seen in morbus Addisonii, and that alone I shall describe. I have never known the supra-renal capsules primarily affected by cancer, melanosis, nor other deposits, excepting the one mentioned; nor do we yet recognise any degeneration of the tissue itself; these have yet to be studied. I speak, therefore, of that change only which is seen to exist in all the following cases, and which, in the earlier ones of Addison, was simply styled scrofulous. The latter term has been very loosely applied to all deposits of a yellow, amorphous, friable character; but whether deserving of the appellation or not, depends much upon the opinion held respecting the nature of tubercle; with some it being a matter of indifference whether the ill-formed cells of such deposit were styled tubercle or simply caco-plastic. Judging from the material itself, it would be difficult to form an opinion of its nature, seeing that a degenerating inflammatory substance would produce a very similar appearance to a scrofulous one, a supra-renal organ so affected being not much unlike the section of a scrofulous lymphatic gland. As regards any assistance to be derived from the general condition of the system, this would rather favour the non-tubercular view of the disease, seeing that it is in only the exceptional cases that any well-marked tubercular deposit has been found in the other viscera. At the very commencement of the discovery in connexion with one of the earliest cases which occurred, I mentioned this fact; and my remarks then made are found appended to Case 7.

“From an examination of the cases before us, it will be seen that when the disease is recent, the organ is somewhat enlarged, and changed into a material which is semi-translucent, of a grey colour, softish, homogeneous, and which, when examined microscopically, is found to be without structure, or sometimes slightly fibrillated, or containing a few abortive nuclei or cells. This lardaceous kind of material is the first deposited, and resembles what is often seen in the early stages of scrofulous enlargement of the lymphatic glands. Subsequently it undergoes a decay or degeneration, as in these glands, and changes into an opaque, yellowish substance, and thus the two materials are constantly found associated—a grey, translucent matter, mixed with a yellow, opaque substance. At a still later period, as in a scrofulous gland, this may soften into a putty-like matter, or it may dry up leaving the mineral part as a chalky deposit, scattered through the organs. These, then, are the changes: first, the deposition of a translucent, softish, homogeneous substance; subsequently the degeneration of this into a yellowish-white, opaque matter; and afterwards a softening into so-called abscess, or drying.

up into a chalky mass. Occasionally, also, some fibrous tissue may be found around the organs, being the product of an inflammation which has united them to the kidney, liver, and adjacent parts.

"It is not sufficiently remembered that some years are necessary for the production of the changes to which we have referred; and therefore, if a cretaceous substance be found in the deposit, there can be no doubt as to the very lengthened duration of the disease. This great length of time corresponds with our knowledge of the duration of the symptoms in some of the best-marked cases, and should compel us to keep under notice suspected instances of the disease for several years.

"It was just now stated, in reference to the question of the scrofulous nature of the disease, that in the best-marked cases there was no appearance of tubercle in any part of the body. In four only was tubercle found in the lungs, and this in two cases existed only as an indurated chronic disease of the apex of the lung, which had a granular surface when incised. In one only was the deposit scattered through the lungs, and in this case it was of so peculiar a nature, that it might be questioned whether it was ordinary tubercle or not. I think, until tubercle can be more strictly defined, that all yellow amorphous deposits ought not to bear the name; and as regards this individual case, the whole character of the lung suggested rather a secondary deposit, as seen in cancer, than one of primary tubercle—an appearance, indeed, which suggested whether the disease in the supra-renal capsule had not been the original affection, and that a secondary deposit had taken place in the lungs of a low organized material, of a similar character, and which could not in strict justice be styled tubercle. When, therefore, it is seen that in most cases there was no evidence whatever of tubercle, and in a few only a little scattered deposit in the apices, as frequently found in post-mortem examinations of those who have died of various ailments, and in one case only where there was abundant deposit, this was of so peculiar a character as to raise a doubt as to its scrofulous character, it will be evident on how slender a basis does this disease in the capsules in morbus Addisonii rest for the name of tubercle.

"I might also observe, that in cases of general tuberculosis, which are very common, I have not yet met with an instance where the supra-renal organs have been affected in the manner of Addison's disease—a very remarkable fact, if the disease should be simply a tuberculous one."

Nor is Dr. Wilks silent with respect to the objections which have been raised against Dr. Addison's observations:—

"These objections," he says, "are mostly of this nature: that if a large collection of cases be made where disease of the supra-renal capsules has been met with, and also that if a large number of cases be collected of discoloration of the skin, and the whole of such cases be put together, those instances where disease of the capsules and discoloration of the skin are combined are so few, that the occurrence cannot be regarded in any other light than a coincidence. This argument has appeared to some conclusive.

"Also that a complete destruction of the capsules, such as Addison has described, has been met with accidentally, or in patients who have died of various diseases.

"Also that the supposed inference of Addison that these organs are vital, is incorrect, since they can be removed from the living animal with impunity.

"At first view these objections may appear important, but to those who have perused the preceding pages, it will be seen that they are of little value. As regards the first objection I have already reiterated the error of Addison, in being not content with the publication of his genuine cases, but in endeavouring to seek minor degrees of the disease, whereby he complicated his subject, and overwhelmed it with much extraneous matter. If, however, as before said, we limit ourselves to that peculiar change in the organs already described, and put aside altogether the cases of cancer, tubercle, and similar affections, we shall rid ourselves at once of the great majority of cases which have been brought against Addison's observations. Thus, in a list of cases published some time since, in not a single instance was the true disease present, but merely those minor changes due to cancer, tubercle, or lardaceous infiltration. As, however, Addison had himself suggested the mistake, no surprise can be manifested when an eminent professor, in reviewing such cases, states that the correctness of Addison's views is still *sub judice*.

"As regards the second argument, that discoloration of the skin has existed without disease of the supra-renal capsules, the fact must be admitted, although I believe that the cases reported are remarkably few where that uniform discoloration has occurred, resembling the mulatto, which Addison described. In most of the cases published to disprove the connexion, the pigmentation has occurred in patches, and in others there can be little doubt that jaundice, pityriasis, ephelis, and ichthyosis, have been mistaken for the true discoloration. It should be remembered, however, that true pigmentation of the skin occurs always in the rete mucosum, and probably may arise from several causes. Addison's statements may be comprised in a few words—that if a pigmentation of the kind described occurred in combination with peculiar constitutional symptoms, certain pathological changes in the supra-renal organs might be inferred, with as much certainty as the diagnosis of any other disease.

"The two remaining objections may be considered together: the objection that these changes in the organs have been met with accidentally—that is, in cases where there was no suspicion of the disease—and also the objection arising from the fact, that their removal from the lower animals may be effected with impunity, showing that they are not vital organs, as Addison's observations would imply. In answer to these, it must be remembered that Addison never stated that they were vital, but that, in all probability, the symptoms of the disease were due to implication of the adjacent organic nerves; and there is every reason to believe that his opinion was correct.

"Addison did not fail to appreciate the difficulty, that whilst in certain cases, when discovering no disease in the body except in the supra-renal capsules he was bound to connect them with the fatal issue as cause and effect, he at the same time was equally certain that these organs had been totally destroyed for a lengthened period, perhaps for months or years before death. He felt the difficulty of describing such organs as vital in the same sense as those viscera in the body whose destruction would inevitably lead to death. If, however, it be true, as Addison surmised, that the symptoms are due to implication of adjacent parts, we can understand how prolonged may be the period of disease, and why it may sometimes be met with quite accidentally; and how also, indeed, the organs may be removed from living animals with impunity. Their excision during life, without manifest symptoms following, is quite in accordance with these facts. It must be remembered that an organ may not be vital, and yet, when diseased, may cause death by implication of other parts; this is true of disease of the ovary, of cancer of the testes and lumbar glands, as well as of the various diseases affecting the extremities of the body, which are not vital parts, but may lead to death when diseased. Why a disease which is equally severe any time during several months should kill on one day rather than another is difficult to explain, but it is a difficulty to be met with in organic diseases of other organs, as the brain, kidneys, &c.

"If, then, we look once more on Addison's disease in its true light, we shall see how all these arguments sink into insignificance. We see a disease which slowly affects the capsules until it has entirely destroyed them; it is not until then, or even after a much longer period, and when still further changes have occurred, that death takes place, and then, perhaps, according to the theory before advanced, by implication of the ganglionic nerves; and this disease is accompanied by an extreme asthenia or feebleness, and generally by a uniform discoloration of the skin. It is not said that every case of extreme feebleness depends on supra-renal disease; nor that every case of discoloration of the skin has the same cause; nor is it said that any deposit in the supra-renal organs is productive of the symptoms just named, but that a peculiar disease in the capsules will be found in cases of general discoloration combined with asthenia, and especially when there is an absence of any other malady. The fact, too, is not incompatible with the statement that a patient may be cut off suddenly by an acute disease or accident, and the total destruction of these organs then be found. Thus are all the opposing arguments futile."

For the cases, and for many other things worthy of notice, we refer to the memoir itself, with this sole additional remark, that few medical writings of the day will better repay the trouble of perusal.

On Diseases of the Chest, including Diseases of the Heart and Great Vessels, their Pathology, Physical Diagnosis, Symptoms, and Treatment.

By HENRY WILLIAM FULLER, M.D. Cantab., Physician to St. George's Hospital, &c.

(8vo. London: Churchill. 1862. pp. 688.)

A treatise on diseases of the chest, written now-a-days, is apt to be received somewhat languidly by the profession. It seems almost as if we had found out all that can be discovered by means of our present rude and imperfect implements of research, and for the rest, that experience is not unfrequently a safer guide to success than the most profound acquaintance with stethoscopy: hence, the first aspect of a new big book upon the subject is little less than repulsive. The name of Dr. Fuller, however, an estimable physician and a careful and diligent clinical observer, will in the present case persuade many persons to read his work—a work which in reality contains a good deal to repay them for their trouble.

With regard to the description of the phenomena of chest diseases and the various means of diagnosis, we may say that this book is one of the best and most complete that we have seen. It gives a very complete summary of the existing state of medical knowledge on these points, and it affords evidence of sound original investigation: while the plan of arranging in parallel columns the symptoms of diseases, which in their general features somewhat resemble each other, so as to exhibit at a glance their real differences, appears to us extremely useful. Those who look out for novelties in physical diagnosis will be interested in Dr. Fuller's observations as to the causes of ægophony—of the cracked-pot sound—and of the increased resonance which sometimes presents itself over condensed lung. The well-known doctrines of Skoda on these points are vigorously, and as we think, successfully attacked; and so far as we can judge, the explanations of these phenomena offered by Dr. Fuller are quite reasonable; while they certainly indicate a large amount of study at the bedside. In short, not to mention further details, the whole of the clinical and pathological portions of the work appear to us to be excellent.

We wish we could say as much for the therapeutical part of the book, for a really good essay on the treatment of chest diseases would be a thing to appreciate highly, notwithstanding all the efforts which have been made in this direction of late years. We cannot fairly say, however, that such a work has been produced by Dr. Fuller, and we think that the majority of our readers will agree with us when we mention the following articles of his creed. He believes firmly in the necessity of bleeding, general or local, and of mercury to salivation, in the large majority of cases of pleurisy and pericarditis, but he does not seem to have had the courage to try the *opium*, which so far as we can see he always adds to these remedies, by itself: although it is well known that many practitioners have

obtained highly successful results from the use of this latter treatment combined with a careful management of diet and general hygiene. With regard to the treatment of pneumonia his doctrines are equally surprising; for he takes occasion to speak of the use of stimulants in sthenic pneumonia in terms which plainly show that he has never had the courage to try, so as to give it a fair chance, one of the most powerful remedial agents with which physicians are acquainted: while all his encomiums are reserved for the use of tartar emetic in sweating and expectorant doses, a method which less than almost any other can be employed indiscriminately with impunity. Such statements as the above are certainly much to be regretted, as they are likely not a little to mislead the inexperienced, coming as they do with the weight of Dr. Fuller's authority. There is something positively alarming in the idea of a relapse, if that were possible, into the dreary barbarism of a routine which prescribed salivation for serous inflammations, and which sought to accomplish by the indirect and dangerous route of blood-letting what might safely and shortly be effected by opium and counter-irritation in the treatment of these diseases. Of course we speak of general principles of treatment, and not of what might possibly be advisable in this or the other exceptional case: and from this point of view we must say that Dr. Fuller's tone surprises us—nay, more, we cannot help thinking that in actual practice he would be found less aggressively antiphlogistic than he appears in his writings, and that a student of St. George's Hospital would be somewhat startled by the rules of practice which his respected teacher promulgates in his present work. In a word, the grand fault of the therapeutic teaching of this book seems to us to be that it implies too great a faith in powerful drugs, and in other heroic modes of treatment, and by far too little trust in the restorative powers of Nature herself. We commend this view of the matter to Dr. Fuller's consideration, and we feel sure that so able an observer, if he will only take the necessary pains, may easily convince himself that much of treatment which he has described as necessary is, in truth, superfluous, if not positively mischievous in the great majority of cases: and that the second edition, which his clever book is sure to reach before long, will contain some important modifications of the views which he at present holds on these points.

The Physiological Method of Treating Consumption.

By Dr. HENRY R. SILVESTER.

(Pamphlet. London: Churchill. 1862. pp. 24.)

Dr. Silvester has made out two important facts in the physiology of respiration, the one pointing to a mode of performing artificial respiration, which is much more effectual than Dr. Marshall Hall's "ready method," the other having, in all probability, an important bearing upon the successful treatment of consumption. The first fact is this—that from nine to forty-four cubic inches of air are taken into and expelled from the chest by pulling the arms upwards

and then bringing them back again to the sides. The second fact is this—that the mere weight of the arms hanging by the sides, particularly in cases where the muscular system is weak, greatly interferes with the free admission of air into the chest, and that full ten additional cubic inches of air are inspired and expired when this weight is removed by grasping hold of something which is at a sufficiently high level. Now Dr. Silvester thinks that this second fact points to a mode of treatment which may be of essential service in the treatment of consumption, and it is the setting forth of this notion which constitutes the chief merits of the pamphlet under notice. With respect to the fact Dr. Silvester says:—

“Whilst performing some experiments on inflating the chest, I found that whilst the upper extremities were hanging down, the body being in an erect position, I experienced difficulty in inflating the upper part of the thorax, and the difficulty was instantly removed by raising and fastening up the arms, so as to take the weight of the shoulders off the chest. This led me to inquire further into the matter. I observed that the two upper extremities balanced each other, and dragged down by their weight the clavicle, sternum, and scapula, so as to oppress the upper ribs, their weight being unopposed by the trapezius, serratus, magnus rhomboideus, levator anguli scapulæ, sterno-cleido-mastoid, etc. This partially accounts for the oppression of the thorax, and the consequent difficulty experienced in inflating the chest. The upper extremity does possess considerable weight; how much, perhaps, we are not prepared to say. I have accordingly subjoined a list of the weight in sixteen cases. Mr. John Wood, assistant-surgeon at King's College Hospital, kindly undertook to ascertain the weights for me. The list is composed of ten male and six female upper extremities. To each about a pound and a half should be added for skin, &c., which might have been removed before the arms were weighed.

		lbs.	ozs.
No. 1.	Male	8	14
2.	Male	6	8
3.	Male	6	11
4.	Male	6	6
5.	Male	8	4
6.	Female	5	6
7.	Female	5	3
8.	Male	8	4
9.	Male	7	2
10.	Male	6	7
11.	Female	6	5
12.	Female	4	12
13.	Female	6	2
14.	Male	6	10
15.	Female	6	3
16.	Male	9	4
This would give an average for			
	Males	7	7
	Females	5	10

"Now, since the weight of the shoulders is, during life, supported by muscular action, general debility, affecting the muscular system by lessening the support, allows the weight of the shoulders to oppress the chest in a corresponding degree."

And again:—

"A few experiments with the spirometer, will tend to show that thoracic respiration may be embarrassed by the weight of the upper extremities.

"I have mentioned above, that whilst performing experiments on the dead body, some difficulty was experienced in inflating the chest, and that this obstacle was removed by taking the weight of the shoulders off the chest. We now have to demonstrate that the conclusions arrived at are borne out by experiment on the living subject; with this view, I have made use of the spirometer, and I subjoin the results obtained, first with the arms depending, and secondly with the weight of the arms removed from oppressing the chest.

Arms down. cubic inches.	Arms up. cubic inches.
150	190
150	190
160	190
180	...
130	...
170	190
175	190
180	190
180	190

"From this table it appears that the amount of inspiration is about ten cubic inches greater when the weight of the upper extremities is removed from off the chest.

"The uniformity of the results contained in the second column appears to be occasioned by the full extent of inspiration having been attained.

"The difference between the two columns appears to show the amount of air entering in this case into the upper lobes of the lungs.

"A state of temporary debility also is observed to diminish the amount of air expired, as indicated by the spirometer. General debility cannot lessen the capacity of the chest for air; this diminution is to be attributed to a loss of muscular power, or want of tone in the muscles of inspiration; they fail to elevate the ribs to the same extent as in perfect health."

With respect to the practical application of this fact, Dr. Silvester says:—

"Various methods of aerating the upper lobes of the lung may be made use of. I will here describe two; one for the standing, and the other for the sitting posture. That for the latter is the more generally useful. A chair, contrived expressly for the purpose, is made by Mr. Heather Bigg, of Leicester-square, London, and will answer the purpose extremely well; it requires no description. An ordinary chair, provided with arms, may be made available for the purpose. The patient, seated in such a chair,

should so place his hands on the arms the chair, as to lift the weight of the shoulders off the chest, and give a fixed attachment for the muscles of respiration.

"Should the patient, however, prefer the standing posture, the following plan may be adopted.

"The patient should be directed to stand erect, and having provided himself with two slender poles, about six feet long, should take one in each hand, and place them perpendicularly, one on each side of him, the lower part of the pole resting on the ground a little in advance of the heels, and raising his hands, clasp the pole as high up as he can conveniently reach, so as to allow the weight of the upper extremities to be borne by the poles, instead of by the patient; then expanding the chest, he should draw a deep inspiration for about the duration of two seconds, and then give a steady forcible expiration for another two seconds—this will allow him to make fifteen deep respirations in a minute, this being about the number per minute made by a person in ordinary health. In order to do this accurately, it is advisable for him to stand opposite a timepiece marking the seconds. When first commenced, the practice needs only to be persevered in for one minute at a time, and repeated three or four times a day. By this method, not only is the weight of the upper extremities taken completely off the chest, but the muscles of respiration having a fixed attachment, the upper ribs are raised by them to their full extent, and pure atmospheric air is drawn into the highest air cells alternately with the expulsion of the products of respiration from the part of the chest most frequently containing the seeds of the disease."

Dr. Silvester says that he has already obtained very satisfactory results in a considerable number of cases, after putting this method of treatment in practice: and this we can readily believe. At any rate, it is only right that we should direct attention prominently to the pamphlet in which the author states his own case in his own way, and this we do accordingly.

Notes on the Possibility of the Embryos of the Guinea-Worm and so-called "Fungus Disease" of India, respectively, entering the Human Body through the Sudorific Ducts.

By MR. H. J. CARTER.

(*Transactions of the Medical and Physiological Society of Bombay.*
No. VII. 1861.)

In his observations on *Dracunculus* (Abstract XXX., p. 144), Mr. Carter expresses an opinion that these parasites are the monster growth of certain *Filaridæ* of the free species, which abound in the Island of Bombay during the "rains," and throughout the year in most of the tanks, and that these embryos pass into the human body through the skin directly, or indirectly through the ducts of the sudorific glands. As yet, however, Mr. Carter has been unable to catch any of these *Filaridæ* in the act of thus entering into the

human body; but he has caught them entering a fungus by analogous apertures on the surface; apertures even smaller than those of the sudorific ducts; and his present object is merely to put on record this interesting fact.

"I have," says Mr. Carter, "formerly stated that the free microscopic Filaridæ chiefly frequent the gelatinous algæ for breeding, and also for food, and now I can add that myriads also accompany almost every species of large fungus for the same purpose. The number of microscopic worms, together with the larvæ of insects, to which the fungi give nourishment, is incredible, and it was on the surface of a large species of *Sphæria* that I observed the fact to which I have above alluded, and of which the following is a description:—

"While examining some specimens of the large digitiform *Xylaria* which grows on the decaying trunks of tamarind-trees, &c., some delicate, glistening, thread-like bodies were seen to project from the summits of the conceptacles (one from each), and to be waving with such an animal motion, that I thought it desirable to ascertain their real nature, so, having collected two or three on the point of a needle for this purpose, they were transferred to a little water on a glass-slide, and placed under a microscope, when they were found to be young Filaridæ—but too undeveloped for their species to be determined.

"The conceptacles are little globe-shaped sacks, imbedded in, and scattered over, the surface of the fungus, upon which they open by minute mouths or ostioles, respectively, which, when measured, were found not to exceed the 1-1880th part of an inch in diameter, so that they are smaller than the orifices of the sudorific ducts of the human body; and from each of these ostioles was projecting a single *Filaria*—the head in the conceptacle.

"If, then, it be possible for these little embryo-worms to enter such small apertures for food in one organic being, it may fairly be inferred that others may do so in another; and hence the possibility, if not probability, of the Guinea-worm in the human subject, being a monstrous development of a particular species of one of the free Filaridæ, which also enters the human body in an embryonal form for food, through the sudorific ducts; assuming as before stated (that, indeed, which is almost a certainty), viz. that the young of *Dracunculus* are too delicate to maintain an independent existence, and therefore cannot propagate the species, which must thus obtain its perpetuity and come from some other source."

A Treatise on Gall-Stones; their Chemistry, Pathology, and Treatment.

By J. L. W. THUDICHUM, M.D., Member of the Royal College of Physicians.

(8vo. London: Churchill. 1863. pp. 315.)

Dr. Thudichum is known in the profession as a pathologist of great ability; indeed, his works on the urine, &c., establish
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his claims to a first-rate position among the investigators of the new school which seeks to aid clinical medicine with all the resources of the laboratory and the microscope. Such a subject as that which occupies him in the work now before us is well calculated to suit his powers, and accordingly he has produced an interesting and valuable treatise.

Dr. Thudichum accounts for the origin of gall-stones in the following manner. He considers the process to be analogous to the formation of the "fusible" urinary calculus, and that it results from a decomposition of the bile (akin to putrefication), which is induced by some cause not yet ascertained, but which is probably a putrid ferment absorbed from the alimentary canal. This causes the amido-acids to split up into their constituents, and—

"Under the influence of a little acetic acid, formed out of glycocholic and some other new acid produced by the putrefactive change, perhaps valerianic acid, choleochrome (the colouring matter of bile), a quantity of cholic acid, and a portion of choloidic acid, together with some salts and a little fat, are deposited. This is the process in the ox, and sometimes in man. But the bile of man differs in this respect from that of the ox, that it contains cholesterine, while that of the ox contains, at the most, only a very small quantity as compared to the other. This cholesterine is dissolved in the taurocholate of soda. But as soon as the acid of this salt is decomposed the cholesterine is set free, crystallizes, and deposits upon any particle that may happen to be within easy distance, in the manner of all crystals, which like to post themselves upon prominent bodies."

This theory appears to be fully sustained by the results of elaborate analyses of putrified bile, both of the ox and of man.

Dr. Thudichum classifies gall-stones, according to their composition, in seven series. 1. Pellucid or pure cholesterine calculi. 2. Mixed calculi with prevalence of cholesterine. 3. Calculi with prevalence of choleochrome. 4. Calculi with prevalence of modified choleochrome. 5. Gall-stones with prevalence of bile-acids. 6. Gall-stones with prevalence of fatty acids. 7. Gall-stones with prevalence of carbonate of lime. The very large number of substances which are found, with greater or less frequency, in gall-stones was a surprise to us, as we dare say it will be to many other readers; among the metals alone no less than five—iron, manganese, copper, mercury, and zinc—exist as occasional ingredients in these connexions; of these, mercury was probably introduced medicinally into the system, but the next cannot be explained away in this manner. A very interesting circumstance which has been noted by Dr. Thudichum is the presence, occasionally, in the biliary nucleus which distinguishes so many calculi, of minute casts of the biliary ducts in the liver, showing that even in the liver itself a mixture of cholesterine and choleochrome must have been deposited, and thus formed a solid substance which when expelled into the gall-bladder attracted additional matters, especially cholesterine. Large and well-executed chromo-lithographs represent the appearance of these casts under the microscope. As might be expected, however,

the casts require searching for very carefully even when they exist, as they are so fragile as easily to be crushed even by the thin glass cover of a microscopic slide.

Among other matters of interest contained in the section on pathology we must instance specially the very complete account which Dr. Thudichum gives of biliary fistulæ. The large number of cases which he has collected of stones which made their way safely through the abdominal parietes is, to say the least of it, highly suggestive, and this the more so as Dr. Harley has spoken of the possibility of artificially forming biliary fistulæ as by no means a merely Utopian project, and has stated his opinion that in many cases of entire occlusion of the common duct, where the gall-bladder was distended, tapping of the latter viscus might safely and beneficially be performed, if only adhesion of it to the parietes of the abdomen were first carefully effected by means of *escharotics*, these being safer than any cutting operation. For our own part, we would venture to suggest the possibility of applying to such cases the plan lately adopted by M. Trousseau with great success, where it was desirable to effect the adhesion of an ovarian tumour to the abdominal wall, viz., the insertion of a number of long needles, within a small circular space, which should hold the gall-bladder and the abdominal wall connected till a solid mass of lymph united them. The entire localization of the inflammatory process in M. Trousseau's cases makes one think that this plan might be safely employed here, while the process would obviously be much less tedious than that by *escharotics*.

The treatment recommended by Dr. Thudichum during the paroxysm is worthy of attention. He strongly recommends that anæsthetic remedies (as chloroform or sulphuric æther) should be administered either by the stomach or by inhalation, in small doses, *repeated till an effect is produced*, the medical man himself remaining with the patient till a favourable change is noticed. Opium he speaks of rather as an auxiliary remedy than as worthy to be depended on chiefly. With this view of the matter we agree entirely, and we are glad to believe that in this country it is coming largely into use. The author rejects blood-letting as useless, and also speaks strongly of the danger of overdoing patients with opiates: he states that when an interval of decided relief has been obtained a purgative must be administered, and he recommends castor oil, or, when there is sickness, seidlitz powders dissolved in a large quantity of water, or even if necessary cathartic pills. Aperient action will often check the vomiting. In cases where any kind of aperient aggravates the sickness, belladonna is recommended (an infusion of nine grains of the comminuted leaves with three ounces of water), or still better, a solution of atropia. With respect to the treatment of gall-stones in the chronic stage, Dr. Thudichum seems to have no great faith in the alleged power of alkalies to effect the solution of any calculi already formed. He is inclined to ascribe the good results which he fully allows are often produced by the waters of Carlsbad, Vichy, &c., to the purgative action which they set up, and the consequent "general revival of all

the abdominal functions by a free circulation." He gives a caution against employing such means when the gall-bladder is distended. Of special alternative remedies to improve the general health and to assist in subduing the remains of jaundice, the author speaks most highly of dilute nitric and of nitrous acid, and of the sp. nit. dulc., which always contains the latter. He thinks that these remedies tend, *inter alia*, to liberate the epithelium-cells of the kidneys from deposits of biliary colouring matter. The chapter on treatment concludes with a vigorous reprobation of the practice of giving mercury, so as to produce its constitutional effects, to gall-stone patients at any stage of their malady.

We have no space to follow Dr. Thudichum into the minute chemical researches with which this volume abounds, and which will render it, no doubt, most valuable as a work of reference. Nor can we do more than simply record the great labour which he has undergone in collecting such a perfect body of historical evidence as he has here presented to us; we can only recognise, with contrition for that comparative indolence which we ourselves, as Englishmen, are naturally distinguished by, that enormous faculty for work, which, save in rare instances, can hardly be generated anywhere but in Germany. Meantime this workman is among us, and we hope that his example will prove infectious, for already we seem to have arrived at a point where it needs but a little more earnest strenuous research to throw a very great and important light on the pathology of liver diseases.

Jaundice: its Pathology and Treatment. With the Application of Physiological Chemistry to the Detection and Treatment of Diseases of the Liver and Pancreas.

By GEORGE HARLEY, M.D., &c. &c.

(8vo. London: Walton and Maberly. 1863. pp. 132.)

This book is well adapted to impress the student of medicine with the necessity, in these days, of following closely the course of physiological chemistry, if he would wish to avoid being left altogether behind the age in regard to the means of diagnosis at his disposal. Especially is this the case with regard to diseases of the liver.

Dr. Harley informs us that he has arrived at substantially the same general classification of the causes of jaundice as was formerly stated with so much clearness by Dr. Budd. That is to say, he holds that all cases of jaundice may be referred either to suppression of the bile secretion, or else to obstruction to its escape into the intestine; and he rejects the ideas of Frerichs as to "abnormal diffusion" of bile from diminished circulation in the liver, and "diminished consumption" of bile in the blood. He thus agrees with Dr. Budd in his conclusions as to pathology, although, as it will be seen, he holds a different theory as to the origin and function of the bile.

According to Dr. Harley, it is the glycocholic and tauro-cholic

acids which are *secreted* by the liver; these are the characteristic elements of the bile, and the only ones which when circulating in the blood prove *poisonous*. The cholesterine and biliverdine, on the other hand, are not *formed* in the liver at all, they are always to be detected in the blood, and the office of the liver in respect of them is merely *secretory*. Therefore, when the secretion of the liver is suppressed, the patient suffers indeed from jaundice, since the pigmentary matters are not eliminated, but there is no poisoning, for the biliary acids have not been formed. But in cases where the bile is secreted, but cannot escape into the intestine (obstruction), reabsorption takes place, and the blood receives not only cholesterine and biliverdine, but the poisonous biliary acids.

It is needless to remark upon the importance of being able to distinguish between two such vastly different pathological states as those above indicated. In practice, however, there is, as every physician knows, the greatest difficulty in many cases in arriving at a satisfactory conclusion; and anything which would indicate a more certain means of diagnosis than we have hitherto possessed would be most welcome. Such a test does really appear to have been provided by the industrious research of Dr. Harley, in the careful application to the urine of jaundiced patients of Pettenkoffer's test with sulphuric acid and sugar. "To a couple of drachms of the suspected urine add a small fragment of loaf sugar, and afterwards pour slowly into the test-tube about a drachm of strong sulphuric acid. This should be done so as not to mix the liquids. If biliary acids be present, there will be observed at the line of contact of the acid and urine—after standing for a few minutes—a deep purple hue. This result may be taken as a sure indication that the jaundice is due to obstructed bile-ducts." If this discovery of Dr. Harley's be confirmed, as it appears likely to be, by the experience of other observers, it is obvious that he has achieved a great thing for diagnosis.

According to the author, the true function of the bile is to enable the fatty matter, previously emulsified by the pancreatic juice, to be absorbed by the lacteals; hence the results of simply preventing the secretion altogether are equivalent to a partial starvation, while the results of impeding the *escape* of the bile when formed are starvation *plus* poisoning with the biliary acids.

Another point of great interest is the value to be attached to the appearance of leucine and tyrosine in the urine. As is well known, Frerichs discovered that these substances were invariably present in cases of acute atrophy of the liver; Dr. Harley has made the interesting observation that they may also be detected in cases of chronic atrophy. Hence it will easily be understood that their appearance after jaundice has already persisted for some time, is a highly important symptom, seeing that it indicates that the liver tissue is itself becoming destroyed by the continuance of the obstacle to excretion. Most important means of prognosis are also afforded, as the author shows, by the quantities of urea and uric acid, and also by the presence of sugar, and the amount of it which exists in the urine. A rapidly increasing quantity of sugar is a sign of very evil import.

With regard to Dr. Harley's teaching as to the treatment of the various forms of jaundice we shall say very little, for we are cautious as to delivering a decided opinion on many of the points which he touches on. One thing, however, we would say—that we think he assumes, without sufficient reason, that podophyllin always tends to excite the liver secretion. Recent observations render it highly probable that the specific action of this drug is upon the mucous membrane of the small intestine, and that action upon the liver is only a secondary consequence, and by no means an universal one; and if these ideas be at all correct, much of the reasoning of Dr. Harley on this point must fall to the ground. Another point which deserves attention is the author's use of benzoic acid as a remedy in cases of jaundice from suppression. We desire very much to hear more about the effects of this remedy, which is being used a good deal just now, as we believe. We cannot say that the instances of its action given by Dr. Harley are particularly conclusive to our minds, but the subject is very interesting, and the remedy deserves to be fairly tested.

A most important suggestion of the author's, and one which we hope will receive the attention it deserves, is the use of bile *administered in a particular way*, as a remedy against the kind of starvation which comes on in cases of persistent obstruction. Dr. Harley states that as usually prepared for medicinal purposes inspissated bile is of little value; he recommends that it should be prepared by simple filtration and subsequent rapid evaporation, and enclosed in capsules; these are to be administered *at the end of stomachal digestion*. By these means the bile will be discharged (the author thinks) into the duodenum in time to meet the chyme at precisely the right moment.

Altogether this is a suggestive and valuable book. We consider that it is likely to give a great and useful impulse to pathology: since it gives convincing proofs of the large amount of good which may be effected by steady practical work in fields which obviously he open to every one.

1. *Lectures on the Distinctive Characters, Pathology, and Treatment of Continued Fevers; delivered at the Royal College of Physicians of London.*

By ALEXANDER TWEEDIE, M.D., F.R.S.

(8vo. London: Churchill. 1862. pp. 301.)

2. *A Treatise on the Continued Fevers of Great Britain.*

By CHARLES MURCHISON, M.D., Fellow of the Royal College of Physicians, &c.

(8vo. London: Parker, Son, and Bourn. 1862. pp. 614.)

There is a great contrast between the two works which form the subject of our present notice. The first of them is not such a treatise

as calls for any lengthened notice, inasmuch as its author can hardly make much pretension to originality. The descriptions of typhus, typhoid, and relapsing fever are correct enough, but there is little freshness about them. It would seem as if modern ideas had rather come to Dr. Tweedie, than been wrought out by his own original researches. And in one point we cannot but believe that he is wrong, viz., in denying the separate existence of fætricular, or as Dr. Murchison has more appropriately called it, simple continued fever. In short, this volume, though respectable as conveying the opinions and experience of a physician who enjoys unusual opportunities for studying fevers, is not such as to excite any very keen interest in the profession.

Dr. Murchison's book is a very different performance. Its author is widely known as an able and untiring investigator of pathological problems, and as especially successful in the field of etiology. In this latter respect his labours have been most valuable, and some of their best results are to be found in this volume, and constitute the highest merit of a work which, however, is good in every part. The exposition here afforded of the causes of continued fevers, and the intelligent view which we are enabled to obtain of the pathological conditions which really distinguish them, are such as to invest the work with the highest interest. Dr. Murchison's introductory chapter on the theory of the Pathology of Idiopathic Fevers is highly interesting: the following summary of it is so convenient that we may be excused for quoting it entire:—

1. "The fever-poison enters the blood.
2. "The nervous system (and particularly the sympathetic and vagus) is paralysed.
3. "The retrograde metamorphosis of the muscles and other tissues is increased, while little or no fresh material is assimilated to compensate for the loss. Increased temperature, great muscular prostration, and loss of weight are the results.
4. "This destruction of tissue is increased by the accelerated action of the heart.
5. "The non-elimination of the products of tissue-metamorphosis gives rise to cerebral symptoms and to local inflammations.
6. "On the elimination of the fever-poison and of the products of tissue-metamorphosis, the nerves resume their normal function, the undue consumption of tissue is checked, and the patient regains his strength and weight. It is impossible to say why this termination occurs at a definite time in different fevers.

"If this be the correct pathology of fever, our objects in treatment ought to be:—1. To neutralize the poison and improve the state of the blood. 2. To promote elimination. 3. To reduce the temperature and the action of the heart. 4. To sustain the vital powers by stimulating the paralysed nervous system, and supplying nourishment to compensate in some measure for the increased consumption of tissue. 5. To relieve distressing symptoms. 6. To obviate and counteract local complications."

It is scarcely necessary to say anything of the classification of fevers adopted by Dr. Murchison, as this has now become familiar

to every one. In ranking enteric fever as an endemic disease depending originally upon fæcal fermentation, we think he is justified by the facts which he brings forward; while on the other hand there can hardly be two opinions as to the correctness of the etiology which he assigns to typhus and relapsing fevers. They both originate from *famine*; but while this cause is of itself sufficient to produce relapsing fever, typhus appears to require for its development the added influence of that kind of poisonous exhalation which taints the air in localities where starved and squalid persons are densely crowded together. We cannot imagine a more interesting occupation for the physiologist and the pathologist than the investigation of such a question as that of the identity or otherwise of typhus and relapsing fever. For our own part, after reading very carefully the evidence cited by Dr. Murchison from various sources, we incline to an opinion the reverse of that which he appears to favour; for we think that there is a decided weight of probabilities in favour of the theory which makes typhus only an aggravated form of relapsing fever. The horrible, yet most interesting, descriptions by Donovan and Kelly of the phenomena observed in the victims of famine at Skibbereen and Mullingar appear to us to furnish a sufficient clue to the whole ascending scale of morbid effects. One has only to imagine the poor wretches whose bodies were covered with a *varnish of fetid secretion* (the odour of which became indefinitely stronger by concentration) crowded together in the courts and alleys of a great city, with no chance of getting any considerable supply of fresh air even during the daytime, and it is surely quite consonant with all the suggestions of reason that an affection, which without this last aggravation might have remained such as Dr. Murchison has described relapsing fever to be, should assume the aggravated severity, contagiousness, and power of protecting the system against a second attack, which are the characteristic features of true typhus. There is, to our thinking, no more striking fact than that of the actual *death in life produced by starvation*, and we would suggest, as an all-important problem for physiologists, the investigation by every possible means of the amount of food, of various sorts, which may prove just so far insufficient for the wants of nature as to give rise to the first symptoms of this strange process. The experiments necessary to determine this point could scarcely be justifiably performed on human beings: but the important researches of Chossat have already proved how much may be done, with regard to this question of inanition, by experiments judiciously performed on animals. The peculiar odour of starvation is probably the first unequivocal evidence of the "purely chemical changes" which Dr. Murchison refers to: this odour is probably caused or accompanied by the exhalation of substances from the skin, which might be chemically examined with very valuable results, such, for instance, as the exact determination of the point of privation from food at which real starvation begins. To return to the question of the identity or non-identity of typhus and relapsing fever, we may remark, that the argument "that the voracious appetite often observed during the paroxysms, and peculiar

to relapsing fever, indicates its more intimate connexion with starvation," is in our opinion liable to be much abused. There is no difficulty, supposing true typhus to represent that *mode* of the starvation fever which is induced by *concentration* of the foul animal odours, in accounting for the greater severity of the nervous depression or (as a secondary result of this) for the want of appetite displayed by persons who are the subjects of it. The facts which Dr. Murchison collects as to the far greater influence exerted by age and by the season of the year on the liability to typhus than on the liability to relapsing fever, appear to us to point distinctly in the same direction.

One thing seems clear—it must be a very considerable *depth* of destitution which is required to produce true famine fevers. For assuredly were this not so, true typhus and relapsing fevers would have appeared in many of the English agricultural districts, where wages are habitually extremely low, and where, occasionally, much distress prevails. The element of over-crowding, even to a very high degree, is by no means wanting, as is often carelessly assumed, in many of those districts. Let those who are practically acquainted with the southern and south-western agricultural counties say, if there be not, in many an outwardly picturesque village, plenty of hovels, (*nay, streets of such hovels*), where the miseries of over-crowding reach as high a pitch as anything that one can see in the worst districts of our great cities. Add to this the consideration that a low agricultural population, though patient (that is, uncomplaining) enough, has little mental resilience, probably far less than the most wretched famine-stricken horde of Irish; this circumstance undoubtedly would favour strongly the depressing influence of famine. Yet in these districts we almost never hear of true typhus, or of relapsing fever, originating spontaneously. So far as we can discover, not even the destitution which prevailed in Wiltshire during the potato famine ever issued in true typhus. Now Wiltshire is the type of a low-paid agricultural county, wages varying, in one large section of it, from 6*s.* to 8*s.* per week, and during a large part of the famine the labourers were unable to supplement their incomes, or their diet, by the help of the produce of their gardens, for the crops of vegetables had failed. Under these circumstances the great majority of families, including the working men, lived upon *white bread alone, or at least upon this and tea, in most cases without sugar or milk*. And there is good reason to believe that one pound and a half of bread per diem was quite the outside of the labourer's ration. In some districts rice, which was sold at 2*d.* per lb., substituted a part of the bread.

Dr. Murchison's treatment of the subject of enteric, or as he prefers to call it, pythogenic fever, is even more able and satisfactory than the earlier portion of the volume. The general tendency of his views has long been known, but in the present work he has elaborated them with a care which leaves little to be desired. The historical sketch which he gives presents a most full and accurate account of the observations of the earlier physicians, and traces the rise of the distinction between enteric fever and typhus in a very

interesting way. He renders justice to Dr. Barlow as being the first to describe this distinction fully, in this country, about the same time that Rochoux was doing the same good service in France, without in the least detracting from the high value of the subsequent researches of Dr. Jenner and others, who finally dispelled the doubts and illusions which remained in the minds of many British physicians. To Dr. Murchison himself remains the credit of demonstrating fully the entirely endemic character of typhoid fever, of dispelling, in great measure, the confusion which had been introduced into the question of the origin and transmission of the disease, and of proving, by the most conclusive statistics, what had been hinted at more or less obscurely before, that contrary to what occurs in the case of typhus, pythogenic fever attacks young and vigorous persons by preference, and that it is greatly more frequent in the autumn season than at any other time of year. With respect to the transmission of typhoid from infected to healthy persons, we confess that his arguments appear to us incontestable, and that the extreme views of Dr. W. Budd and others must be considered as finally set aside: the careful investigation of the circumstances under which the epidemic at Windsor in 1858 occurred seems to us to yield sufficient evidence of this in itself. We shall not, however, attempt to analyse this part of the volume, but recommend the reader to study it carefully for himself.

With regard to the section on simple continued fever, we must own to some little dissatisfaction: the subject seems to us too hastily and briefly treated, when it might have been made much more complete and interesting. We should have been particularly glad to see a full and particular account, with clinical illustrations, of the "ardent fever" of the tropics, and as Dr. Murchison himself had large practical experience of it when in India and Burmah, we think it a pity that he did not supply more exact information. We are particularly dissatisfied with the extremely curt mention of the subject of treatment; for although Dr. Murchison's work professedly deals with British fevers only, this very question of the proper treatment of the tropical ardent fever bears a most interesting relation to cerebral affections attended with febrile symptoms, which are observed in this country, and it is important, therefore, that every means should be taken to arrive at a correct idea of the treatment which is really the best for the former diseases. If it really be true that there is all the difference which our author states to exist between the simple continued fevers of Britain and of the tropics, it would be most interesting to know the opinions of so acute an observer as to the cause of this difference. We are the more urgent in pressing this on Dr. Murchison's attention, because we have heard it strenuously insisted, by more than one able surgeon of Indian experience, that in fact this difference does not exist, and that an immense deal of the "lancet practice" so common in that country in the treatment of ardent fever is mere unnecessary routine, and is even very injurious in many cases.

Dr. Murchison's observations on the treatment of typhus, relapsing, and pythogenic fever are highly valuable, not so much,

perhaps, because they establish any very novel principles, as that they carefully review the ground on which the modern system of treatment is based, and the sources of the errors which from time to time have led to the adoption of unnecessary or hurtful measures. The exposure which is given of the source of the mistake which led to the extensive employment of blood-letting in continued fevers generally, in consequence of the supposed success of this method of treatment in the Irish fever of 1817-19, is very satisfactory. The cases treated with such apparent success were instances of relapsing fever, a malady which tends strongly to spontaneous cure. The blood-letting was therefore superfluous; and on the other hand, abundant proof has been supplied that in cases of true typhus bleeding is entirely mischievous.

With the general principles of treatment recommended by the author we are inclined to agree, and we think there is much force in the distinction which he draws between typhus and enteric fever, as to the necessity for support, and especially for stimulants. There are one or two points, however, in which we find ourselves at issue with Dr. Murchison; and we desire, with all deference to so high an authority, to express our views as to these matters.

Dr. Murchison is very explicit and peremptory in rejecting indiscriminate stimulation (a plan of treatment which we are not aware has ever been recommended); and in particular he attacks the dictum of Todd, that stimulants are invariably called for in cases where delirium appears. He believes that there is no good reason for treating alcohol as a food; and he believes that there is at least one kind of delirium—*delirium ferox*—in which alcohol is not useful, but positively noxious.

Now as regards the claim of alcohol to be called a food, that is a question as yet much disputed. We are quite ready to go with Dr. Murchison so far as to admit that the old theory of Liebig can no longer be maintained in its original form; at the same time we beg to remind him that the arguments brought forward in the article to which he has referred (*London Medical Review*, 1862) did not take that theory as its basis of argument at all, but relied exclusively on carefully recorded facts, and upon the circumstance that the much-talked-of researches of Dr. Smith and of MM. Lallemand, Duroy, and Perrin have only proved that when alcohol is taken into the body in poisonous doses a portion of it is always eliminated by the skin, lungs, and kidneys. Against these latter somewhat meagre results it is not necessary to fight very hard. It is quite sufficient to adduce the well-known habits of large numbers of pot-boys, draymen, &c., whose occupation, and many others whose choice leads them to support themselves almost entirely upon alcoholic drinks while leading very active lives, to show that *primâ facie* there is the strongest probability that alcohol is capable of acting as food in some way or other. That it is highly improper and injurious to use it during health in the way that it is used by habitual toppers is certain; but to say that it acts in these cases simply as an anæsthetic, in the face of facts like those we have referred to, is simply to make an assumption quite unsupported by

facts. The physician who employs alcohol in typhus fever because he thinks it acts as a food is by no means under an obligation to receive Liebig's theory, as Dr. Murchison quietly assumes. Food means that which unaided will sustain life, and sustain at the same time the energy of the vital processes for a longer period than they could support themselves in the absence of all ingesta: this is the only philosophic definition of the word; and consequently the arguments against the transformation of alcohol in the body do not touch the main question. If Dr. Murchison doubts the possibility of a substance which is neither transformed in the body nor converted directly into tissue under any circumstances, to act as food, we recommend to his attention Von Tschudi's account of the effects of coca-chewing upon the Peruvian Indians, in enabling them to go very long periods of time without any ordinary aliment while working extremely hard. It is, we think, impossible for any candid person to dispute that the effects there described are those of a true food, and as they are in strict analogy with a large number of facts which have been recorded respecting the action, under similar circumstances, of alcohol, they certainly afford a very strong justification of the belief that during fevers and acute diseases it is probable that alcoholic drinks may substitute with good effect the ordinary food, which owing to the disturbances of digestion cannot be taken. So much for the general question; as to particulars, we beg to protest very strongly against one notion of Dr. Murchison's—that flushing of the face under the use of alcohol shows that the treatment is unsuitable, and that the alcohol ought to be withdrawn. If Dr. Murchison had said *that it ought to be given in smaller doses*, he would have been quite right. Flushing of the face indicates paralysis of the sympathetic, one of the early symptoms of the poisonous action of alcohol, but it does not by any means prove that smaller quantities will not act beneficially, but only that we must not throw such large doses *at once* into the circulation.

As for the cases of “delirium ferox” which will not bear the use of stimulants, we can only say that in our opinion this symptom would never have been attributed to the subjects either of typhus or typhoid but for the influence of one-sided and prejudiced observation; and with all deference for Dr. Murchison's ability and extensive experience, we cannot but think that even he has been somewhat affected in this way. We can honestly say that, in our own experience, several cases which have occurred bore precisely the features which he describes as “delirium ferox,” in which the delirium came on after many hours had been expended in fruitless efforts to get the patient to take any food of the ordinary kind, and was at once quieted by wine or brandy. In short, we believe that mere violence of muscular effort, injection of the conjunctivæ, and “boldness” of countenance, proves little or nothing as against the advisability of administering support and stimulus. Who so “bold” as the sufferer from acute mania? And who more readily tamed by food and by alcoholic stimulants?

Further, and lastly, we protest against the rule being laid down that alcoholic stimulants are not to be employed when the skin is

hot and dry. Whatever else may be an indication against the use of alcohol, this certainly, when taken alone, is not; and we must say, that with the vivid recollection which we have of many cases bearing on this point, we are amazed that Dr. Murchison should repeat this exploded fallacy.

In the objections which we have thus made to Dr. Murchison's views with regard to the administration of alcohol, we by no means desire to recommend an indiscriminate use of this stimulant, or to deny that its employment has been pushed to an absurd extreme by some thoughtless persons. We merely desire to do away with terrors which are the result of mere theorizing, and to ensure that the revision, now going on, of the principles on which alcoholic stimuli should be administered shall be fair and honest.

And now we have nothing more to do except to thank our author, for ourselves and for the whole profession, for one of the most masterly medical treatises in the English language. Dr. Murchison has built himself what we venture to say will prove a lasting monument, and future generations will doubtless recognise his labours as marking the decisive turning-point at which the subject of fevers emerged from comparative obscurity and confusion into the light of a sound and rational pathology.

The Spas of Europe.

By JULIUS ALTHAUS, M.D., Member of the Royal College of Physicians, &c. &c.

(8vo. London: Trübner and Co. pp. 476.)

The subject of natural mineral waters is an important, and in this country a much-neglected one; and Dr. Althaus has done good service in providing what is really a thoroughly good book of reference in all that relates to it. This is no volume of superficial gossip, but a good solid, scientific work, in which the whole question of the action of mineral waters, physiological and therapeutical, is carefully treated, and the chemical constitution of these waters thoroughly investigated. For all who require information on the subject, as nearly every English medical man does, we recommend this book as a guide to the information which they seek, such as has certainly not hitherto appeared in our language.

Health in the Tropics; or, Sanitary Art applied to Europeans in India.

By W. J. MOORE, Licentiate of the Royal College of Physicians, Edinburgh, &c.; Bombay Medical Service.

(8vo. London: Churchill. 1862. pp. 304.)

The author of this work is already known to the profession through the *Manual of the Diseases of India*, which he has published, and

which has met with a favourable reception. His present work is an interesting and useful one, and will doubtless be read not only by medical men, but also by many civilians, and let us hope by all statesmen who have anything to do with the management of our civil and military establishments in tropical districts, particularly in India. The author is evidently practically acquainted with his subject, and has treated it with great care and fulness. So many subjects of interest are treated of, however, that it is impossible for us to criticise the work in detail ; and we must therefore leave that task to the reader, merely adding as a caution that the chief value of this volume is in its record of clinical and vital facts, and that in regard to a few points of physiological chemistry which are introduced, it can hardly be said to display an amount of information which fairly represents the science of to-day. It will, however, well repay every one for the trouble of its perusal.

II.

REPORT ON SURGERY.

*Lectures on Surgery, delivered in St. Bartholomew's
Hospital.*

By WILLIAM LAWRENCE, F.R.S., Serjeant-Surgeon to the Queen,
and Surgeon to the Hospitals of St. Bartholomew and
Bethlehem.

(8vo. London: Churchill. 1862. pp. 632.)

Mr. Lawrence's *Lectures on Surgery*, though a new book, is really little else than a reprint of those lectures which appeared in the *Lancet* thirty years ago. The first twenty pages are occupied by an Introduction, which we imagine must, at some time or other, have done duty on the 1st of October, as an Introductory Lecture, since it abounds with those truisms, historical references, and good advices which form the staple of such exciting discourses. Mr. Lawrence is very urgent on the point of the science of medicine being "one and indivisible," which is rather surprising, inasmuch as he is one of the most influential members of the Council of the College of Surgeons, which rejected contemptuously all overtures of union, as regards examinations, &c., on the part of the College of Physicians.

INFLAMMATION leads the van, *more majorem*, and here let us venture a protest against the absurd practice of beginning two and sometimes three different courses of lectures to the same students with this same subject. The lecturer on medicine, the lecturer on surgery, and the lecturer on pathology (if he exists) give accounts of this complicated action, each in his own way, and according to his lights, with the result of confusing, rather than clearing, the minds of his auditors. Would it not be possible for the lecturers on medicine and surgery to give the principles of the subject in alternate years, and perhaps with advantage? The author does not expend much labour upon the subject, however, for in ten pages he describes its nature and leading symptoms, embodying in half a page "the general opinion of the day," with the comment, "which I do not pretend to indorse," and expressing his belief that "it would be a complete waste of time to examine further the various hypotheses respecting inflammation"!

In the treatment of inflammation, Mr. Lawrence is, as might be expected, an upholder of the *ancien régime* as regards bleeding, calomel, &c. That he views inflammation as a sthenic affection is sufficiently obvious from the whole of the chapter, but we are not left in doubt for want of plain speaking; *e.g.*, "It has been objected to the large bleedings which we sometimes find necessary, that they weaken. They certainly do so in one sense; that is their object. We think that the patient has too much blood, and that the heart is acting too powerfully; that he is too strong, if those circumstances are the criteria of strength."

We almost doubted at one time whether Mr. Lawrence had ever heard of any other than modern views with regard to inflammation, but we see by the following that he has, and are content to take his opinion for what it is worth:—

"That the practice of taking away blood occasionally, to a greater or less extent, in the treatment of inflammatory disorders, which has prevailed universally wherever the science and art of medicine have been most successfully cultivated, may have been altogether a mistake, is possible. Stronger evidence and arguments than have yet been produced will be necessary to prove the point, and still more powerful proofs will be required to satisfy us that the opposite mode of combating such affections, by means of stimulants, including the free use of brandy, is either advantageous or safe. It would be a public misfortune if the sanction of professional authority should be given to a treatment calculated to encourage the pernicious habit of spirit-drinking."

We will take our leave of this part of our subject with an extract embodying much sounder views than that last quoted, one which is emphatically "common sense," and which both advocates and opponents of the depletory system would do well to ponder on:—

"You will not, I trust, misunderstand my observations so as to suppose that I recommend the loss of blood, or particularly general bleeding in all cases. Remember that I have been speaking of acute inflammation, attended generally with inflammatory fever in cases of importance, either from the nature of the affection or the extent of the mischief. There are many inflammations in which it is not necessary to abstract blood either generally or locally; there are others in which local bleeding answers every purpose, in conjunction with other means. You must consider, in particular instances, the importance of the affected part, the degree of disturbance, the probability that continuance of the inflammation may alter structure and impair function, also the presence or absence of constitutional disturbance. You must bear in mind the age, constitution, and habit of body, the general powers of the patient, and regulate your choice of means by a combined view of these several particulars. When it is your object to arrest inflammation suddenly, on account of the importance of the organ, or the immediate urgency of the symptoms, or from apprehension of those structural changes which inflammation often produces, you will find it advantageous to employ general bleeding, and to take at once such a quantity of blood as will influence the circulation. You will thus arrest inflam-

mation more effectually and at less expense to the constitution than by the repetition of smaller bleedings."

SUPPURATION and ABSCESS are clearly dealt with by Mr. Lawrence, who comprises under seven heads nearly all the cases in which surgical interference is necessary, but which space will not allow us to quote in detail.

ULCERATION, SLOUGHING PHAGEDÆNA, and HOSPITAL GANGRENE are comprehended in the ninth chapter, but the views propounded present no novelty. Mr. Lawrence's opinion with respect to the contagiousness of sloughing phagedæna will be best learnt from his own words:—

"The question of contagion naturally presents itself to the mind in viewing this serious disease, and the unpleasant secretion which accompanies it. That the discharge does not affect the sound skin of the patient is clearly seen from the way in which it has been applied to nearer and more remote parts of the body before admission into hospital. It would, therefore, probably not affect the skin of a healthy person, and hospital nurses have never been injured by it, so far as I know. Does it contaminate the atmosphere, so as to act injuriously on previously healthy sores in patients occupying contiguous beds, or lying in the same ward? We entertain suspicion that it has such influence, and act on the opinion by keeping patients who labour under the affection as far apart from others as possible."

MORTIFICATION is described in the usual manner, but Mr. Lawrence seems to have only just made up his mind that the process by which a dead part is separated is *bona fide* ulceration, and not absorption, as suggested by "Mr. Hunter."

Among the various *causes* of mortification, the author mentions scarifications in anasarcoous legs, which he characterizes as "a practice altogether unnecessary." We venture to differ with him, for though, no doubt, mortification does *occasionally* ensue, the relief to the general symptoms afforded by the proceeding in cases of dropsy can be scarcely too highly estimated. The vexed question of amputation in traumatic gangrene is very clearly treated by Mr. Lawrence, who says:—

"If we wait until the mortification stops and the line of demarcation between the dead and living parts occurs, the patient surely dies; for the mortification does not become limited. In deciding on this point we have to consider not only the state of the part, but also that of the constitution. In an unhealthy habit a slight injury may cause traumatic gangrene, which is owing in such a case to the badness of constitution rather than to the local mischief. Such a constitution would form one of the most cogent reasons against the operation."

And again a few pages farther on:—

"I have no hesitation in saying that in patients of healthy constitution, where gangrene arises from external causes simply, you must disregard the rule of waiting for the line of demarcation."

From his extensive experience Mr. Lawrence's opinion upon a subject as the treatment of tetanus is worthy of attention.

He states plainly his belief, that amputation in cases where the disease is supposed to depend upon a local cause is useless, but acknowledges that with regard to general treatment he is almost at a loss for a guiding principle. Free purgation with croton oil seems to be regarded by him with favour, and the case recorded appears to have been benefited by the treatment, although unfortunately its final result is not stated.

SYPHILIS AND GONORRHOEA occupy one hundred and sixty pages of Mr. Lawrence's work! He goes at some length into the vexed question of the origin of syphilis, and then proceeds to examine the question of unity or duality of poisons, believing himself that there is but one syphilitic virus. With regard to the *diagnosis* of syphilitic sores, the following pithy remarks are worthy of notice:—

"The doctrines promulgated of late years respecting the peculiar characters of true venereal sores, respecting syphilis, and the diseases said to resemble it without being venereal, that is between syphilis and pseudo-syphilis, have occasioned much needless perplexity to practitioners who are unable to recognise, in the majority of cases, the characters ascribed to the genuine disease. They were pursuing a chimera, and were inevitably disappointed and perplexed. The affair, in truth, is very simple. When ulcerations occur in healthy individuals at an interval varying from a few days to a much longer time after connexion with females who receive a plurality of visitors, they are syphilitic in ninety-nine cases out of a hundred, or a still greater proportion, whatever the characters of the sores may be. The criteria of primary syphilis, therefore, would be sexual intercourse with women leading a life of prostitution, or at least unchaste, and the manifestation of the disease after a longer or shorter interval during which the part has been perfectly healthy. A further evidence, which the lapse of time only can afford, is the occurrence of secondary or constitutional symptoms. When this has been added to the two former circumstances, the disease is syphilis, whatever may have been the character of the original sore."

This, it must be acknowledged, is rather unsatisfactory teaching, and one which hardly comes up to modern views upon the subject, but it is remarkable that though we find in these chapters abundant references to Hunter, Pearson, Carmichael, and others, we nowhere find mention of Ricord, Lee, or other modern authorities!

Some doubt is likely to arise in the mind of the reader from the uses Mr. Lawrence makes of the words "sloughing" and "phagedænic." He describes first that form of sloughing (or phagedænic) primary sore which is ordinarily treated by splitting up the prepuce and by the free application of escharotics, but at a later page he speaks again of the phagedænic sore as distinct from the sloughing sore and as requiring mercury.

That there is a form of ulceration of a syphilitic character, which requires mercury for its arrest, we are quite aware, but it appears to us to be likely to lead to error in practice if we apply to it the term phagedæna, which to most minds conveys the idea of a non-syphilitic disorder. Mr. Lawrence's own definition must be carefully borne in mind in reading the following passage:—

"As the phagedænic and sloughing sores are allied forms of disease, the one passing by invisible gradations into the other, we may be at a loss to determine whether to administer mercury, or to adopt a mild and soothing local treatment. When the sore presents a blackish or dark brown slough, it is a case for soothing local measures, perhaps with some direct antiphlogistic treatment; if the surface be grey, white, or phagedænic, mercury may be advisable. In a doubtful case, if there is local inflammation, with febrile constitutional disturbance, try antiphlogistic means; if these fail, employ mercury decidedly; should that aggravate the symptoms, opium should be used internally and locally, with iodide of potassium and sarsaparilla."

The remaining hundred and thirty-two pages are devoted to the consideration of the various forms of cancer. Here the author's prolonged experience is of service in illustrating his remarks with numerous interesting cases. We shall do no more, however, than quote his observations upon the operation for the removal of the cancerous mamma—observations with which we heartily agree:—

"The most favourable time for the operation is when the disease is in the indolent period, though frequently we do not see it till that time is past. If the breast has become fixed to the chest, if the skin covering and surrounding the gland is thickened and hardened, and if cutaneous tubercles are formed in the neighbourhood, the operation cannot be recommended. Disease of the axillary glands, which can be removed, and the commencement of ulceration, are not conclusive against operating, if the breast is loose upon the chest, and all diseased parts can be effectually taken away. To refuse operation is to pass sentence of death on the patient, and that of the most painful, distressing, and often lingering kind: few minds are strong enough to bear this. To delay and temporize, wasting time in treatment which is sure to be fruitless, keeps up a state of uncertainty and anxiety, with indefinite and exaggerated apprehensions of evil, which depresses the spirits, adding unnecessarily to the difficulties of the case, and often leading the sufferer to take refuge in quackery. I consider it a strong reason for operating, particularly if the breast be large, that it prevents the possibility of death by ulcerated cancer, the greatest calamity that can befall a female. The operation is neither painful nor dangerous; the wound heals, and death may ensue after an uncertain length of time from some internal affection, not usually of much suffering or long duration."

Thus then we bring this notice to an end. As a volume embodying much experience it may be useful as a work of reference upon a few subjects, but for the student (for whom lectures are generally intended) the information is scant on very many important points, whilst the doctrines inculcated are in many particulars not those with which he is expected to be familiar.

Outlines of Surgery; being an Epitome of the Lectures on the Principles and Practice of Surgery, delivered at St. Thomas's Hospital.

By F. LE GROS CLARK, Esq., Surgeon to the Hospital, &c.

(London: Charchill. 1863. pp. 258.)

This work, as its title implies, is only an epitome of surgery, and therefore it must not be expected to enter fully into all the details of the nature and treatment of every surgical disorder. Being without woodcuts, too, it does not attempt to show the various modes of treatment by apparatus, &c.—a matter which occupies so important a place in many of our surgical manuals. In these particulars Mr. Clark's work is not unlike that useful old book, *Cooper's First Lines*, and it is adapted for the same uses, viz., as a manual for the use of the junior student, or as a reminder for the more advanced pupil or junior practitioner.

In one or two points we do not agree with Mr. Clark. We do not agree, for example, in believing that "deep structures may be severely injured without breach of surface, by the 'wind of a ball,' i.e., the sudden vacuum caused by its near and rapid transit;" nor that "a bone forcing its way through the skin generally involves more contusion of the soft parts than the penetration of a foreign body from without;" but for the most part the descriptions and directions for treatment are accurate and to the point.

The following extracts relating to matters of everyday importance will give a good idea of the general scope of the work :—

"A *strangulated hernia* may be of small or large size, but the symptoms are little influenced by its magnitude. The characteristic signs of strangulation are local and general. The local symptoms are the presence of a tumour, more or less tense and tender, in a position where rupture may occur, with pain extending over the abdomen, and especially in the umbilical or epigastric region. The general symptoms are, obstructed bowels (usually), nausea or sickness; and, in a more advanced stage, stercoraceous vomiting, hiccup, general abdominal tenderness and distension, with a quick, sharp pulse, hot skin, restlessness, and an anxious countenance. Cessation of local tenderness and collapse mark the still later stage of mortification of the contents of the sac. The local signs may resemble abscess, inflamed glands, malignant tumour, or undescended testicle; the general symptoms are likewise indicative of intussusception, or of mechanical obstruction from internal strangulation, from the presence of malignant tumour, or from impacted fæces. The presence of both local and general symptoms, together with the history, rarely leaves room for doubt.

"The *taxis* should be first attempted, by flexing the thigh, and making firm but gentle pressure on the tumour, whilst the forefinger and thumb of the free hand grasp the neck of the sac so as to direct the rupture in its proper course: violence is inadmissible; and the continuance of this manipulation must be determined by circum-

stances. Gentle, uninterrupted pressure will sometimes empty the intestine, which is indicated by a gurgling sensation: this often precedes the reduction. A lesson may often be learned by directing a patient to attempt the return of an old hernia, when strangulated. Sometimes a rupture may be reduced by elevating a patient's thighs and pelvis, and thus compelling its return by the gravitation and traction of the viscera within the abdomen. If these means fail, warm bath or chloroform *may* be tried; but they rarely succeed, and have the disadvantage often of inducing sickness, and susceptibility to cold from subsequent exposure. More confidence may be placed in the continuous application of ice to the tumour, which acts as a powerful astringent; but this is not always admissible: a full injection of warm water sometimes accomplishes the desired relief; and the same effect has followed the diligent use of hot fomentations. An operation should not be delayed; it is far better to operate early, where there is doubt, than to subject the patient to the extra risk of delay: ill-success rarely attends the former error, if such it be, but frequently the latter."

"We will suppose that a surgeon is summoned to a case of retention of urine, without being acquainted with any particulars respecting the case: how should he proceed? He must first learn whether the patient has been the subject of permanent stricture; and if so, in what sized stream his water was passed. He will then inquire particularly into the nature of the present attack; how it was induced, and how long it has lasted. He will ascertain the degree of distension of the bladder by feeling the hypogastric region. Having satisfied himself of the simple nature of the case, he will proceed to relieve the patient.

"But, the patient may be past middle age, and the symptoms may be those of enlarged prostate rather than of stricture. If such prove, on examination per anum, to be the case, a suitable instrument must be selected and introduced. This operation, if properly conducted, is in many instances very simple. The erect posture is the best. A large catheter with a long curve, made either of gum or silver, will often enter the bladder without difficulty; care being taken, if a metal instrument is used, not to depress the handle too soon: whereas a smaller and shorter instrument must stop short of its destination; and, if force be employed, it is very likely to perforate the gland, and to be followed by copious bleeding. It is best to try first a gum elastic instrument without the wire, as it is the safest: but if that fail, after firm but gentle pressure has been tried, then the stilet may be introduced, or a silver catheter substituted. Smaller instruments may, if necessary, be subsequently tried; but their shape and length must be the same. The introduction of the finger into the rectum will serve at once as a safeguard and to guide the point of the instrument, as it enters the prostatic portion of the urethra. Unless there be some unlooked-for complication, it rarely occurs that steady and gentle perseverance fails of success. But this may be the case, and the urgency of the symptoms may not admit of temporising, even with opium. If admissible, a dose of opium may be given to allay pain; and then, after an interval, the

attempt may be renewed. If still unsuccessful, the case becomes serious and perplexing, for the prostatic enlargement forbids an attempt to puncture the bladder through the rectum: opening the perineum would afford no assistance; and puncturing the bladder above the pubes is an undesirable proceeding, if there be any other alternative for giving relief. Yet, the only other course open to the surgeon, in this otherwise hopeless dilemma, is to force a passage through the gland, which can be accomplished by firmly pressing a metal instrument onwards, taking care that its point is rightly directed towards the bladder. When introduced either by the natural or through an artificial opening, the catheter must be kept in until the bladder has recovered its tone; except in those cases, which are numerous, in which there is but little or no difficulty in performing this operation. Then the instrument may be withdrawn after relief has been given, and introduced at intervals, to draw off the water. This is often required for a lengthened period; and it may be desirable to instruct the patient in the use of the instrument, that he may relieve himself.

"If the symptoms indicate the presence of a stone impacted in the urethra, the surgeon may satisfy himself on this point, by carrying his finger along that passage, from its orifice to the prostate gland. If he find one, he will proceed to remove it, in accordance with the directions and with the precautions already given. If, perchance, it should be impacted far back in the prostate, it must be removed by an incision similar to that for the lateral operation, stopping short of entering the bladder. It is not easy to feel a stone in this position: but it may be struck by a catheter; or the instrument may be felt grating against it, as it is pressed onwards into the bladder: but this condition, as a cause of retention, is not of frequent occurrence. Still less frequently is a stone within the bladder a source of absolute retention: and if so, it would probably be displaced by the introduction of an instrument.

"But the retention may be due to other causes. The patient may, indeed, have been the subject of stricture, but he will say that he has always been able to pass his water in a stream, until he felt pain in the perineum, a few days previously: and this may have succeeded an awkward attempt to introduce an instrument, to relieve the stricture, or it may have been quite spontaneous. He would, possibly, add that he had a shivering fit, and subsequently had felt very hot and feverish. His suffering from inability to micturate is greatly enhanced by the pain in the perineum, and by his thirst, which he dreads to satisfy, as he will thereby add to the accumulation in the bladder. On examining the perineum, the true state of the case is apparent, and a prompt and deep incision into the inflamed tissues, and the liberation of the pus, which may perhaps be but a few drops, will afford relief.

"The case may, however, have taken on the more serious aspect of diffuse extravasation of urine into the scrotum, by the sudden bursting of the urethra behind an old stricture, in, probably, a disorganized urethra. Sloughing is inevitable in this condition; but its extent and the chances of the patient's recovery depend on the

early period at which free incisions are made. In some instances, not only is the areolar tissue in the hypogastrium implicated, but a large quantity of urine may be found collected in sloughy pouches in the scrotum and perineum. The constitutional disturbance and suffering are proportioned to the intensity of the inflammation and destruction of tissue: and yet, with attention to cleanliness, ventilation, and security for the escape of the urine, together with a liberal diet and suitable stimulus, it is remarkable what a good recovery many of the worst cases make.

"Lastly, the surgeon may find the cause of obstruction to be purely accidental. It may be laceration of the urethra, existing as a complication of fracture of the pelvis, or from a fall across some angular and hard body, or from a kick in the perineum. It is well, in such case, if the patient has either not made an attempt to micturate, or has failed in the effort. The immediate introduction of a catheter is required: for, the longer it is deferred, the more difficult it is likely to become, in consequence of the increasing ecchymosis in the perineum and around the urethra. The instrument—an elastic one is preferable—when introduced, must be retained for at least two or three days before it is changed: and then another should be cautiously substituted, and worn until the condition of parts proves that it can be safely dispensed with, which is generally, in simple cases, after the lapse of a week: its presence, when no longer requisite, is likely to excite irritation in the canal."

Anatomy of the Parts concerned in Femoral Rupture.

By GEORGE W. CALLENDER, Esq., Assistant-Surgeon to, and Demonstrator of Anatomy at, St. Bartholomew's Hospital.

(8vo. London: Churchill. 1863. pp. 51.)

This little volume, without any great pretence to originality, shows some little anatomical, and a great deal of antiquarian research. Mr. Callender's great anatomical point is that the "saphenous opening" in the fascia lata is not a true opening with a "falciform margin," &c., but that it is the result of an artificial dissection by which sharp margins are defined. No anatomist, we imagine, is unaware of this, but at the same time it is well known that there is a weak point in the fascia lata, call it by what name we will, through which a femoral hernia finds its way, and the margins of which are occasionally sufficiently rigid to cause strangulation at that point. What, therefore, is the practical advantage of ignoring the existence of the aperture, we confess, we are unable to see. The author has appended very profuse notes to his main treatise, showing the several authorities from whom we derive the terms now commonly in use with regard to the anatomy of the groin, and also the several names which have from time to time been applied to the same parts by different anatomists. Since so much confusion has arisen from this practice of re-naming parts, we are surprised to find Mr. Callender

proposing the term "sub-peritoneal fascia" for that tissue which is by all modern authorities called "fascia transversalis"!

Observations on the Diseases of the Rectum; with Wood Engravings.

By T. B. CURLING, Esq., F.R.S., Surgeon to the London Hospital,
Examiner in Surgery to the University of London.

(Third Edition, Revised and Enlarged. London: John Churchill & Sons.
1863. pp. 232.)

This is a third and enlarged edition of Mr. Curling's very able work on the Rectum—a work, the value of which is, we believe, fully appreciated by the profession. If for nothing else this work deserves the highest praise for the healthy tone prevailing from beginning to end. Mr. Curling has introduced several new chapters in this edition, and more especially, he has investigated the nature of the several malformations ordinarily massed together under the term "imperforate anus." On this point the information afforded cannot fail to be of service to the practitioner in the treatment of these difficult cases. The author prefers the operation in the groin to that in the loin in cases where the surgeon has failed to find the rectum from the perineum, and shows by his dissections that the position of the colon in the loin in these cases is by no means certain. In the adult, on the contrary, Mr. Curling gives the preference to the lumbar operation—an operation which he has performed four times, and twice with success.

The other new chapters treat of the following subjects:—Nervous affection of the rectum; the villous tumour; epithelial cancer of the anus and rectum; atony of the rectum; organic contraction of the anus; obstructions of the rectum, and operations required for their relief. We extract that upon a not commonly recognised disorder—the villous tumour.

"*Villous Tumour of the Rectum.*—A growth similar to the villous tumour which occurs in the bladder and on other mucous surfaces, sometimes forms in the rectum. It springs from the mucous membrane generally by a broad base, is soft in structure, and composed of a number of projecting papillæ or villi. On minute examination it is found to vary in structure according to the proportion of the fibrous or vascular elements entering into its composition. A characteristic specimen in the London Hospital Museum is described by Dr. A. Clark as essentially an outgrowth of dense areolar tissue, permeated by blood-vessels, and assuming a papillary form; the papillæ being flattened and curled so as to represent hollow cylinders, and being clothed with layers of epithelium, the free layers being cylindrical. Its minute structure indeed closely resembles the soft polypus. The villous tumour is innocent in character, and is not apt to return after complete removal. Its chief peculiarity in the rectum, as in the bladder, is a remarkable disposition to bleed. This growth

occurs only in adults, and is a rare disease. When it projects at the anus, it exhibits the characteristic projecting processes of a deep red colour.

“Mr. Syme has described two cases of soft vascular bleeding polypus of the rectum, which, I presume, was this disease. He states, that he removed from a hospital patient a tumour not less than an orange, which had nearly exhausted the patient by hæmorrhage. In another case, in which the disease was detected from the great hæmorrhage which it occasioned, he tied the tumour within the rectum. The villous tumour has not generally a peduncle or neck. It has been particularly described by Mr. Quain under the name of a ‘Peculiar Bleeding Tumour of the Rectum,’ but as it closely resembles the outgrowths found in the bladder, usually called villous, I prefer the latter term. Mr. Quain met with it in two cases in females, one middle-aged, the other sixty-eight. The largest tumour of the kind which I have seen was in a man of middle age, a patient at St. Mark’s Hospital, and was shown me by Mr. Gowland. The growth was attached to the lower part of the rectum by a broad base, and projected at stool. It had been forming for some years. This tumour was successfully excised by operation, and it is the one already referred to as examined by Dr. A. Clark. The bleeding to which the villous growth generally gives rise, and the slimy discharge, render its removal very necessary. If the tumour be attached high up, and a ligature can be applied round the base of it, this is desirable, as it would be difficult to arrest the bleeding after excision.”

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On the Cure of Club-Foot without Cutting Tendons ; and on certain New Methods of Treating other Deformities.

By RICHARD BARWELL, Esq., F.R.C.S., Assistant-Surgeon,
Charing-cross Hospital, &c.

(Illustrated by Engravings on Wood.)

(London : John Churchill & Sons. 1863. pp. 224.)

In 1861, Mr. Barwell brought under the notice of the Medico-Chirurgical Society the question of the propriety of the universal division of tendons in cases of club-foot, as practised in the present day by orthopædic surgeons, and this work is the embodiment of his views and experience in this matter.

With the view of showing the evil effects of wholesale tenotomy Mr. Barwell has examined the records of all cases which have died after such an operation, and from these data he proves incontestably that so far from tendons reuniting invariably, as laid down by Mr. Adams and others, it is, with the exception of the *tendo Achillis*, the exception for the two divided ends to reunite. He proves, in fact, that these ends but too frequently contract adhesions to the surrounding fibrous sheaths. Mr. Barwell then

investigates the rationale and results of tenotomy, and endeavours to show that the principle of the operation is false, since it is directed rather to paralyse those muscles which still act, instead of assisting the action of those heretofore paralysed; and that after these operations, although it is quite possible by means of various apparatus to squeeze the now passive foot into something like proper shape, yet the patient, owing to the loss of power caused by the surgeon's proceedings, never recovers the proper use of the limb. The author's "new method" consists in assisting the weak and paralysed muscles to take on their proper action, rather than in destroying the action of their healthy antagonists; and this is done by india-rubber springs so arranged as to correspond in direction with, and therefore to simulate the action of the affected muscles. These springs are affixed to the foot by means of strips of plaster, which strips correspond as closely as possible to the true insertions of the muscles, whilst they get their fixed point from a tin splint affixed to the limb. We must refer for the ingenious variations of the simple means which Mr. Barwell has contrived for the treatment of each form of club-foot to the work itself, merely stating that the results of the detailed cases seem to be very satisfactory.

In addition to this novel method of treating club-foot the author has turned his attention to the deformities, "knock-knee," "bow-legs," and "crooked-shins," and has most ingeniously adapted the use of metallic springs to their treatment. The principle of treatment, modified of course to suit each case, is to bend a piece of flat steel spring to the exact curve of the deformed limb by means of a clamp, which clamp is kept in situ until the spring has been firmly fastened to the limb by means of plaster. On withdrawing the clamp the spring at once resumes as far as possible the straight position, and thereby drags the limb into its normal condition. In every respect, indeed, this treatment appears to be more promising than the old method by rigid splints, bandages, and buckles.

We have derived great satisfaction from the perusal of Mr. Barwell's unpretending but important work, and we commend it heartily to the profession at large, as a work which is likely to exercise a revolutionary influence upon orthopædic surgery. We append one of Dr. Barwell's cases to illustrate his treatment:—

CASE X.—G. S., aged 14 months, was brought to me with a congenital varus of the left foot, Sept. 27th, 1862. On account chiefly of family reasons, the foot has not been attended to; but at this time Mrs. S. was very anxious that the deformity should be cured. The varus is of the highest degree, the tarsus being very much twisted, and inclined inwards, so that there was a deep furrow on the inner side, and the great toe nearly touched the tibia. The front part of the sole faced backwards, and the cuboid would have been the point of support for the body, had the patient been able to walk. That bone was the most projecting part of a very sharp convexity on the outer side of the tarsus. The tendo Achillis formed a curve with the convexity, looking inwards, but such curve was very slight when compared to the twist of the anterior tarsus. The head of the astragalus was quite distinct; it was very fairly developed and round. Endeavouring with my hand to put the foot as much into form as possible, I found the

resistant power very considerable. Explained that much time would be saved by giving this child chloroform, and by the action of the hands placing the foot as nearly as possible in the normal posture. At the same time, that the limb would not remain in that position, but must undergo further treatment.

October 2nd.—Chloroform was administered, and during the time of its inhalation a gutta percha splint moulded on the limb. As soon as the influence became marked, the splint was removed, and when anæsthesia was fully established the foot was straightened in the following manner:—The heel, the malleoli, and the posterior part of the foot were firmly held in the left hand, to prevent any possible twist being given to the ankle-joint. The front part of the foot was grasped in the right, and brought slowly towards a normal position. By the exercise of but very slight power, the foot could be straightened very much nearer its true position than it could have been brought previous to the exhibition of chloroform by the exercise of great force; however at a certain point, there was a decided check to any further replacement. A quick, firm, yet by no means violent movement of the right hand, overcame this resistance with a slight sound. Some adduction of the front upon the back half of the foot was then the only remaining malposture; between the ball of the great toe and the heel was the usual sharp edge of a tight band—a couple of slight jerks overcame the resistance in this part. By the above methods the foot was reduced into a shape that was very nearly normal, as long as held in the proper posture, and even when released it returned but very little towards the malposture. Before the child recovered from the influence of the chloroform, the splint, having been thickly padded with cotton-wool, was re-applied; the part of the foot not covered with the splint was also protected and kept warm by a thick cushion of wool placed next the skin, and held in position by the bandage.

5 P.M.—The child was fretful and the skin was a little hot; but the evidences of pain were very slight. A hot bath was ordered, of course with the proviso that the foot must be kept out of the water.

4th.—Yesterday, the child, who is naturally unquiet, was rather more fretful than usual, but not markedly so. To-day he seemed quite in his ordinary condition. I removed the splint and examined the foot. The position was the same as the original deformity, which is, of course, attributable to its having been put in a splint of that form; but the limb, instead of being rigid, was loose, and could be turned easily; however, as motion outwards appeared to cause some pain or uneasiness, the splint was re-applied.

6th.—Neither rotation outwards nor adduction, even to a position very near the normal, appeared to cause any pain. The springs were applied after the manner detailed.

15th.—On the 10th a shorter india-rubber spring had been supplied in the place of the peroneus longus. The foot was this day in tolerable position; the sole looked directly downwards, but the front half of the foot was somewhat adducted on the back part. The appliances were re-adapted, and more tension exerted upon the catgut, representing the tendon of the peroneus brevis.

29th.—The child's foot was at this date very nearly restored to a normal posture. Still shorter springs were employed, so as to reverse the deformity, and he was to be encouraged to try and walk.

November 18th.—The foot was very much better. The springs employed were so short as either to evert the foot, or to place it into a valgus position; however, when the springs were removed, the limb fell slightly

into the verous posture. He could walk a little ; indeed, considering that he had never attempted it before, he had made considerable progress in the past fortnight.

December 20th.—The child had been diligently taught to walk, and the foot had gained greatly ; in fact, it was now straight, and acted normally in walking without the springs. However, the apparatus was applied for the last time to-day, the foot still put rather into the valgous condition.

Case of Iliac Aneurism remedied by opening the Sac, and tying the Common Iliac, the External Iliac, and the Internal Iliac Arteries.

By Mr. SYME, Professor of Clinical Surgery in the University of Edinburgh.

(*Proceedings of the Royal Medico-Chirurgical Society, May 27, 1863.*)

Two years ago Mr. Syme endeavoured to show reason for regarding the preference given to the Hunterian operation for aneurism as too exclusive, since the circumstances which rendered it advantageous in cases where the popliteal artery was affected might be so different in other situations as to reverse the grounds of choice—as in axillary aneurism, where the vessel is easily reached at the seat of rupture, and with great difficulty, as well as danger, tied above the clavicle. In reply to the doctrine usually taught, that the artery is not in a condition suitable for the ligature so far as the aneurismal sac extends, it is remarked that the size of the tumour does not depend upon the state of the vessel, and therefore cannot be taken as any measure of the extent to which its coats are impaired ; while the formation of a sac, so far from proving injurious to the artery, must rather tend to strengthen and support it by consolidating the textures in its neighbourhood. Trusting to this view of the matter, the author has operated with success in various cases, and he now relates one of recent occurrence, which seems strongly corroborative of the opinion he has expressed.

CASE.—R. L.—, a seaman, thirty-one years of age, towards the end of November last received a blow on his left groin, which caused a painful swelling that was treated as glandular ; and about a month afterwards strained himself on the same side in leaping from a ship that was about to sink, with the effect of causing the formation of another tumour some inches higher up than the former one. On this account he repaired to the Cumberland Infirmary, at Carlisle, and was then found to suffer from an iliac aneurism. After remaining a week in the hospital, he went home until the end of February, when he was admitted into the hospital of Dumfries. Operative interference having been there declined, the patient again went home, and betook himself to intemperate habits, with the effect of injuring his health and greatly increasing the tumour.

On the 18th of April he was admitted into the Royal Infirmary of Edinburgh, when the aneurism was found to extend from below Poupart's ligament considerably higher up than the umbilicus, and from two inches beyond the middle line of the abdomen towards the right side, completely

across the left iliac region, so as to overlap the crest of the ilium. There was a strong pulsation throughout the whole extent of the tumour, great pain in the course of the crural nerve, and considerable œdema of the thigh.

On the 20th, chloroform having been administered, the cavity was examined by introducing first one finger, then another, and finally the whole hand, without any trace of the artery being detected, whence it was concluded to be out of its usual situation. A screw clamp provided by Professor Lister, of Glasgow, was then employed to effect compression of the aorta; and this having been ascertained to be complete, a free incision was made through all the textures concerned, so as to lay the sac fully open, and allow six pounds of blood and clots to be scooped out. It then appeared that the arterial orifice was in the roof of the aneurism, from the vessel having been raised in this direction by the blood effused under it; and this orifice, being brought distinctly into view by dissection of the sac, was tied on both sides of the vessel. But as blood still issued, though not with the same force as before the ligatures were applied, it was concluded that the internal iliac originated from the portion of artery comprehended between them; and this vessel also having been exposed, was tied by a thread passed round it. The wound was then dressed superficially, and everything went on favourably. On the nineteenth day the ligatures separated, and the cavity gradually contracted.

After relating this case some observations are made—1. On the importance of ascertaining that the aorta could be effectually compressed so as to prevent hæmorrhage from its primary branches. 2. On the sac not maintaining a profuse and protracted suppuration like the investment of a chronic abscess, but readily contracting so soon as the distending force ceased to act. 3. On the impossibility of affording relief in the case related by any other means than the one employed, and the danger which would have attended ligature of the common iliac at an earlier period from the aperture being so near the bifurcation.

In conclusion, the author expresses hope that the cases of carotid, axillary, gluteal, and iliac aneurisms which have been under his observation would induce teachers of surgery to reconsider the propriety of representing the Hunterian operation as so exclusively the rule of practice as it has hitherto been regarded.

On the Mortality after excision of the Knee, as practised hitherto in London.

By Mr. T. HOLMES, Surgeon to the Hospital for Sick Children, and Assistant-Surgeon to St. George's Hospital.

(*Medico-Chirurgical Review*, July, 1862).

In this paper, Mr. Holmes supplies information of a very startling character, namely this—that the operation of excision of the knee, instead of having been less fatal than amputation of the thigh, has turned out in general practice certainly, and, in the practice of the London hospitals most probably, more than twice as fatal as that

operation, and that in both classes of cases its failures appear to have been more numerous than its successes. The information supplied is obtained partly from an excellent essay on "The Excision of the Joints," by Dr. Richard M. Hodges, recently published in America, and partly from data which have been collected by Mr. Holmes himself from the records of the various metropolitan hospitals. Dr. Hodges, in his essay, has brought together from all sources no less than 208 cases of excision of the knee for chronic disease. Of these cases, in round numbers, one-third died; more than half are known to have failed; and there is no direct evidence of success in more than one-third of the cases, even accepting the statements of those who furnished the notes. The exact numbers are these: 60 were directly fatal without amputation, and 9 others were known to have died after amputation; the whole number of those who underwent amputation was 42; in 14 other cases the limb is stated to have been more or less useless:—thus, making the whole number of known failures 116. In 27 other cases there is no information on the subject of the usefulness of the limb, the simple statement of "recovery" being all that is given. In the remaining 65, the accounts furnished represent the patient as having obtained a useful limb. The mortality taken at 69 is a minimum, since of the 42 amputated only 35 are accounted for, the result being left uncertain in the other 7 cases. The number of failures, if taken at 116, is also a minimum, since it is all but certain that some of the 27 unaccounted for must have failed; while, in admitting the fact of success in the 65 cases in which it is claimed, we are leaving out of view the great tendency of advocates of a given plan of treatment (and nearly all those who furnished the notes are in this position) to overestimate the success of their favourite plan, as well as the great frequency of occurrences which afterwards spoil a union that seemed useful at first, such as gradual yielding and flexure backwards or sideways, recurring disease, interrupted growth in childhood, &c.

"No one," says Mr. Holmes, "would think of comparing such success as this with the results of amputation in metropolitan hospitals; since it has been clearly proved that the rate of death in amputation of the thigh, at its lower part, for chronic disease of the knee, is about one-seventh of the number of cases—and of those who recover, hardly any are not relieved from local disease, irritable and diseased stumps being rare in this amputation. Several objections, however, may be made to the comparison between Dr. Hodges' statistics and those of amputation in London hospitals, all of which objections may be included in the statement that the two series of cases were not under the same conditions. But no such objections could be raised against a comparison instituted between a complete series of the cases of excision of the knee-joint treated in the metropolitan hospitals, and those of amputation of the thigh in the same institution, since the patients were of the same class, subjected to the same influences of habits and constitution, under the same surgical treatment, and in the same atmosphere. I had wished, therefore, to procure satisfactory data of all the cases of excision treated at all the metropolitan hospitals, including the following under that

term: St Bartholomew's, Guy's, St. Thomas's, King's College, St. George's, University College, the Westminster, the Middlesex, the London, Charing-cross, St. Mary's, the Hospital for Sick Children, the Great Northern, and the Royal Free Hospital.

"I much regret, however, that the defective method in which statistics are kept at nearly all our hospitals (I might say, the utter absence of all attempt to keep such statistics in many of them), has disappointed me in this attempt. Still, though baffled in my attempt to obtain complete statistics, I have succeeded in procuring a considerable list of authentic cases—in fact, one which includes the very great majority of the operations which have been performed at the above-mentioned hospitals. This list may be taken as nearly correct for the number of deaths in the cases comprised in it, and it may perhaps, without serious error, be accepted as showing the percentage of deaths which would prevail in the series, if it could be completed. I believe, also, that the number of secondary amputations is correct for the cases included in the list. But the details which would show the usefulness of the limb in those cases which recovered—which I regard as the most important point of all—are, I am sorry to say, not to be obtained in sufficient numbers to allow of any sound deduction being made. Hence, although I have to thank the surgical authorities of all the hospitals for the greatest courtesy and liberality in allowing me to make use of such materials as they possess, I am compelled to say that these materials are insufficient, and that the following list will give only negative results—i.e., it will show the minimum of ill success which may have attended the operation as practised in London, but will give no reliable data as to the number of useful limbs which were really turned out by the 95 operations of which it is composed. It may be interesting to add the numbers, as showing the great diversity of opinion which must prevail among hospital surgeons as to the propriety of performing the operation. Of the 95 cases, 32 are from King's College Hospital, 14 from St. Thomas's, 13 from University College, 6 from the Great Northern, 5 from St. George's, 5 from the Westminster, 3 from St. Bartholomew's, 3 from Guy's, 3 from Charing Cross, 3 from the Hospital for Sick Children, 2 from the Middlesex, 2 from St. Mary's, 2 from the London, and 2 from the Royal Free Hospital. Of these 95 patients, 27 are known to have died; and in 10 others the operation failed, as shown in 8 cases by amputation, and in the other two by the limb being reported as useless, though it is not known to have been removed. This would make the minimum rate of mortality 28.4, and the minimum rate of failure 38.9 per cent. But out of the 58 remaining cases the accounts of 10 are either unsatisfactory, leading to the suspicion that the limb was not useful, or are entirely vague; while of 19 of the others, forming a portion of the King's College series, I have only the most summary account; and although I know that many of these operations succeeded, some, I have no doubt, must have failed. Hence we may suspect that no better success has attended the operation in the hands of the hospital surgeons of London than in those of other operators, and that as many of the cases have failed as have succeeded.

"Now, it is by no means my intention, in writing these few lines, to dwell on the failures of a novel method of treatment, still less to decry or to endeavour to banish from practice an operation which, on the contrary, I hold, in properly-selected cases, to be extremely useful. I only wish to show how very fallacious the statements which have been hitherto made as to its relative mortality, when compared with that of amputation, have been; how uncertain we really are what benefits have resulted in the so-called successful cases; and as a consequence from these facts, which, I presume, will soon be patent and admitted by every one, how urgently needed is some better plan of keeping the records of our great hospitals, if the ample experience which might be collected from them is to be made available to the great body of the profession. The exaggerated and inconsiderate assertions which were made of the incredible success of excision of the knee by its earlier and warmer advocates, have done much to discourage more sober-minded practitioners from an operation which, had it not been unduly extolled, would sooner have come into more extended, though perhaps less indiscriminate use; and these assertions would probably have never been received as true, had authentic hospital statistics been at hand by which to test them. At present, the operation is in danger of being discredited by failures which perhaps are not essential to its performance. We are told, and with great probability, that the recent mortality after the operation shows signs of diminishing, that the selection of cases fitted for the operation is now better understood, that the importance of certain rules for the performance of the operation and its after-treatment, is now recognised and acted upon, and so that fewer patients will die, and those who recover will do so with more useful limbs. I sincerely hope that it may be so, and I am anxious to believe that it will; but we cannot forget that we were told far more confidently, as a matter of fact, a few years ago, that the operation *had* proved more successful in both these particulars than amputation; and if that assertion had any foundation in fact, the present altered condition of things can only have been brought about by the cases of excision having turned out *less* successful recently."

Practical Lithotomy and Lithotrity; or an Inquiry into the best modes of removing Stone from the Bladder.

By HENRY THOMPSON, Esq., F.R.C.S., Assistant-Surgeon to University College Hospital, Consulting Surgeon to the St. Marylebone Infirmary, &c., &c.

(London: John Churchill & Sons. 1863. Pp. 274.)

The greater part of this work has already appeared in the pages of the *Lancet* as the "Lettsonian Lectures" for 1862; but this fact has not done away with the necessity for publication in a more convenient and accessible form. On the contrary, the articles in the

journal have only served to bring out this necessity for separate publication in the most conclusive manner possible.

The description of the anatomy of the parts concerned in the operation of lithotomy is both accurate and to the point. In this part we do not expect much novelty, but there is one point which is certainly new, and which we commend to anatomists and accoucheurs—namely, this, that the proper simile for the male pelvic outlet is neither the triangle nor the lozenge, but the conventional heart—the ace of hearts, with the apex upwards.

The description of the instruments necessary for lithotomy, and of the operation itself, is clear and accurate, and the discussion upon the advantages and disadvantages of the various modes of operating is well calculated to bring out the truth. With respect to the operation which has lately been revived extensively—Allerton's median operation, Mr. Thompson says:—

“Of all the cutting operations for stone, it is unquestionable that the median still presents that in which the bladder is reached with the smallest amount of section by the knife. And it appears to become dangerous just in proportion as injury by laceration, or over-pressure under the name of dilatation, is superadded to the incisions. These latter involve the bulb to a small degree, which is the only structure of importance divided by the knife besides the prostate, and this latter is only notched at the apex in the ordinary mode of performing the operation. But when the deeper parts feel more than usually rigid and unyielding, or when the stone proves to be larger than was anticipated, it is advisable to make an incision in the left side in the same direction as in lateral lithotomy, but generally less extended, for the purpose of affording space. This is accomplished after the urethra has been opened, by introducing a long straight probe-pointed bistoury, guided by the left index finger, and made to incise as much as the operator deems necessary. Unless this or some similar proceeding be adopted, the opening is certainly small and feels tight to the finger, and if the stone is large, appears to me to require an additional incision.

“After all, the anatomical axiom laid down at first must not be forgotten, viz., that any operation the incisions of which lie altogether in the line above the anus and below the symphysis pubis, unless aided by a lateral section, never can afford an opening sufficiently capacious for the removal of very large stones without dangerous laceration. Examine the pelvic outlet, and contrast the want of space in this situation caused by the converging pubic rami, with the room which exists in one of its lateral divisions, and the correctness of this assertion will, I think, be manifest.”

We fully agree with Mr. Thompson in believing that the danger of infiltration of urine after lithotomy is much exaggerated, and that, when after death suppuration and infiltration are found, the urinary extravasation is not the primary cause of the inflammation, but *vice versa*.

“Infiltration by no means necessarily occurs when urine passes over the newly-made section of cellular spaces so called. In fact, cellular interspaces between muscles and between viscera do not

exist except when made by the anatomist for the necessary purpose of demonstrating the planes of cellular tissue which unite adjacent organs, and facilitate freedom of movement between them. I very much doubt if urinary infiltration ever occurs when they are otherwise uninjured, in a person of fair vigorous health. To judge from the language held respecting this subject, one would imagine that hollow intervals existed between the organs in question, over which urine had only to be poured in order to be drained mechanically into them. No such thing exists. In the child, where the cellular connexions are of the loosest and most delicate kind, and where the bladder is active, powerful, and irritable, urine flows constantly after this operation over a cut surface, which affords free access to them; nevertheless, with what extreme rarity do we meet with urinary infiltration in the child. But once inflame this cellular tissue, or even perhaps let the patient be of unsound health, or one in whom 'the flesh never heals well,' to use a common phrase, and then we have the condition in which urinary infiltration may take place with rapid and fatal effect."

That fatal inflammation may arise from too violent efforts to drag a large stone through a small aperture in the prostate is abundantly shown by Mr. Thompson, and at the same time slow and steady traction is recommended, in place of violent though brilliant manipulations. In the case of difficulty from the size of the stone in a median lithotomy, as we have already seen, the author advises the conversion of the median into the lateral operation; whilst in the case of the lateral, he shows that an incision into the right side of the prostate will readily convert the lateral into the bilateral operation, and thus allow abundance of room for the extraction of even a very large calculus.

With regard to the "high operation," Mr. Thompson is of opinion that there are but few cases in which, owing to the want of expansion of the bladder consequent upon the irritation of the stone, it can be justifiable; and he gives some post-mortem experiments upon a patient who died with an enormous stone in his bladder, with a view to showing that it would have been impossible to reach the viscus without damaging the peritoneum.

In the latter part of the volume is a very able and full account of lithotripsy in all its varieties. The author traces the rise and progress of this operation, and then insists strongly upon the necessity of accustoming a patient's bladder and urethra to the use of instruments before commencing a series of lithotritic sittings. A very minute description of the several forms of lithotrite in common use is given, with many woodcuts as illustrations; and after this the author goes on to show what he considers to be the best mode of proceeding for first finding and then breaking up the stone. The "English" mode, so strongly advocated by Brodie and his followers, consists in bringing the calculus to the instrument, *i.e.*, in depressing the base of the bladder with the lithotrite so as to make the stone fall between its blades. The contrary proceeding is the one to which Mr. Thompson gives the preference, *viz.*, to search the bladder carefully with the litho-

trite, and having found the calculus, to seize it with the instrument. This, the author says, "is the mode of Civiale," and no doubt it is so, but we should have liked to see some notice of the fact, of which Mr. Thompson can hardly be ignorant, viz., that Mr. Fergusson has employed the very same method throughout his career; and that this eminent surgeon has many imitators among his pupils.

In order to obtain success in lithotrity, it is essential that the surgeon should not merely have regard to the health of the urethra and bladder, and to the general condition of the patient, but that he should have arrived at a clear understanding as respects the size and nature of the stone itself, before deciding upon any operative proceeding. Upon this subject Mr. Thompson lays down this important canon:—"I repeat it, then, it is probably safer uniformly to practise lithotomy in every instance, if the surgeon does not arrive at an accurate diagnosis of the nature of the stone, and select an operation in accordance with it."

We observe that Mr. Thompson is rather opposed to the use of chloroform in lithotrity, not for the absurd old reason that it was necessary to have the patient sensible in order to avoid nipping the coats of the bladder, but for the more rational reason that, as the operation itself, if properly performed, is a nearly painless one, any evidence of excessive irritability or tenderness on the part of the patient is an indication for the suspension of any operative proceedings—at all events, for a time. A large amount of statistics has been collected by the author in order to contrast the fatality of the two operations at different ages, and unusual pains seem to have been taken to exclude sources of error. In a word, we cannot but regard Mr. Thompson's work as a valuable contribution to the literature of an important subject, and one in every way worthy of the reputation of the author.

III.

REPORT ON MATERIA MEDICA AND
THERAPEUTICS.*The Alcohol Question.*

By Dr. FRANCIS E. ANSTIE, Assistant-Physician to Westminster Hospital, and Lecturer on Toxicology to the School.

(*London Medical Review*, February and March, 1862.)

Is it Food, Medicine, or Poison? By the same.

(*Cornhill Magazine*, June, 1862.)

Does Alcohol act as Food? By the same.

(*Cornhill Magazine*, September, 1862.)

Dr. Anstie describes—I. The immediate action of alcohol upon the nervous system.—II. Its remote action upon the tissues generally as regards their nutrition.

I. Action upon the nervous system. Alcohol, when taken into the stomach in any dose, immediately enters the circulation. It is distributed more especially to the nervous system, which has some special affinity for it (Lallemand, Duroy, and Perrin). The symptoms which follow differ accordingly as the dose is small or large, and accordingly as it is taken into the system slowly or rapidly.

1. Effects of a small dose.—The pulse is strengthened, but not made quicker unless it had previously been unnaturally slow; if too rapid (from debility) its frequency is reduced. The temperature of the surface is comfortable, and there is a slight feeling of *glow*, but no flushing. The activity of the brain is increased, an alacrity for exertion is produced, the feeling of fatigue disappears, also the morbid depression of spirits which is the result of fatigue. Tendencies to convulsive muscular action are diminished. This train of symptoms indicates that the brain, the spinal cord, and the sympathetic ganglia have been stimulated, and their circulation rendered healthily active. After a certain length of time these effects gradually subside, leaving matters as they were before the dose, except in cases where decidedly morbid depression had existed previously to the dose; in such cases a decided improvement on the

original condition is often permanently maintained. There is no morbid after-depression as the ultimate result of a small dose of alcohol.

2. Effects of a large dose *taken slowly*—In the first place, the symptoms of healthy excitement are produced; for as yet only a small dose has entered the blood. But as soon as the blood becomes impregnated with a certain proportion of alcohol a train of symptoms commences which are radically and essentially distinct from those previously observed, viz., the symptoms of narcotism. Alcoholic narcotism is equivalent to *paralysis*; every one of its symptoms is a consequence, direct or remote, of palsy of some portion of the nervous system. The sympathetic system suffers first, as indicated by flushing of the face and great heat of the surface, which leads quickly to sweating. The cerebral hemispheres follow next, or rather these and a portion of the medulla oblongata are simultaneously affected. The mental phenomena are curious: first of all (in certain instances, though these are not frequent) there is a kind of involuntary memory of past events; simultaneously with this, or in its absence where it does not occur, perceptions of time and space are impaired; and shortly afterwards the reasoning faculty is seriously interfered with: as a consequence of this last the drunken person is often extravagant and violent in words or acts, the passions being now uncontrolled. The subsequent mental phenomena are those of progressive obliteration, down to the point of entire unconsciousness. The first indications of paralysis of the medulla oblongata are exhibited by the muscoli motores oculi, by the muscles of the tongue, and by the sensitive branches of the fifth pair which supply the lips. Spinal paralysis now commences (or in some cases earlier); the posterior limbs are first affected; superficial sensation and the co-ordinative power are somewhat earlier affected than voluntary motion. When complete paralysis of the limbs has developed itself, either the patient lies in a state of unconsciousness and immobility for some time, and then slowly recovers, or if the dose have been very large, palsy of the respiratory muscles sets in (profound affection of the medulla oblongata), and breathing at last ceases, the heart continuing to beat and the intestines to contract for some time longer.

3. Effects of a large dose taken rapidly.—Supposing a very large quantity of alcohol to be swallowed at *once*, and no obstacle to exist to its rapid absorption, no stimulant action occurs, for the blood is rapidly impregnated with a narcotic dose. The phenomena are from the first those of paralysis, and in extreme cases so strong an impression may at once be made upon the nervous centres that death may ensue in a minute or two.

Dr. Anstie argues that the first of these series of actions upon the nervous system—that of the small dose—is in no sense analogous to, or a part of, the train of narcotic symptoms induced, sooner or later, by a large dose. He insists that it is only the narcotic action which produces collapse, attended with irrepressible craving for stimulus, a feeling never experienced by the ordinary moderate drinker; the latter steadily continues to take the same small dose

every day, with the same effect. He distinguishes between the ensnaring influence of pleasant effects on the palate, which may lead to excess in any food, and true physiological craving, the offspring of nervous depression. He draws attention to the fact that many substances have an action on the body in small doses *totally different in kind* to that which they exercise in large doses (*e.g.*, common salt, arsenic, and many others, which are either foods or poisons, according to the dose).

Dr. Anstie next observes that in many acute diseases, such as fevers, and many inflammations, also in severe hæmorrhages, &c., the "poison line" is evidently shifted. This is proved by the fact that symptoms of narcotic poisoning are not induced by doses which in health might even prove fatal. Recovery from the disease is accompanied by an intolerance of these large doses: *but there are none of the symptoms of recovery from the collapse of narcotic poisoning.* In a healthy person these symptoms, after a debauch, are such as are well known, and they could not escape attention if they occurred during the convalescence from disease which had been treated with large doses of alcohol. Where they do occur, they are a proof that alcohol has been used in excess, or unnecessarily. Further, it is to be noted that where alcohol has been used in large doses in disease, and has not produced intoxication, the patient, on recovery, never experiences a morbid craving for drink. It is evident, then, that this latter can only be produced as a result of distinctly *poisonous* alcoholic action; and, on the other hand, that the beneficial influence of alcohol in acute disease is not owing to a narcotic effect.

II. Influence of alcohol upon the general nutrition of the body.

1. It may be conceded that a portion of the alcohol which we take into our bodies is quickly eliminated, unchanged, by skin, lungs, and kidneys.

2. It is not proved that the whole or even the larger part is so eliminated. With regard to the experiments of Lallemand, Duroy, and Perrin, the following points are to be noted:—(a) In all the animals whose blood and tissues were analysed, alcohol was given in enormous doses. The animals were dogs, the doses from 90 to 300 grammes. (b) In no case was it proved that the whole of the alcohol had left the body. (c) On the contrary, although in no case could alcohol be detected in the secretions later than from eight to fourteen hours after the dose, in one case where the animal survived for sixteen hours plenty of alcohol was found in the brain, liver, &c. (d) This residual portion might be destined to transformation in the body.

3. It must be conceded that the experiments of Masing, and more recently of Lallemand, throw grave doubts on the accuracy of the observations of Bouchardat and Sandras, of Duchek and others, who supposed they detected aldehyde, acetic acid and oxalic acid in the blood of alcoholized animals.

4. It is by no means proved that a residual portion of the alcohol taken into the body, after some sojourn in the tissues, is not transformed into carbonic acid and water.

5. Persons who habitually drink to excess eat little solid food.

6. The ordinary effect of such a course of life is to produce gradual degeneration of the tissues (*e.g.* muscle becomes fatty, secreting structure of glands atrophies, peculiar cells of nervous centres wither away).

7. But in a not inconsiderable number of cases no such result happens. Cases of habitual toppers living to old age are quite common, and these persons nearly always take only the smallest quantities of solid food. Besides very many cases of this kind, Dr. Anstie has collected the facts of two cases, one observed by himself, another by trustworthy friends, in which alcohol was the only aliment taken except a little water. A man who died at the age of eight-four drank daily, for his last twenty years, a bottle of gin, and took no other food, except a minute portion of bread: he was active in his habits; and enjoyed good bodily health. A gentleman engaged in active business was liable to outbreaks of seeming insanity, in which (although usually sober) he would drink spirits and water continuously for weeks together, taking all this time no food but the alcohol; yet at the end of these periods he never appeared emaciated. Of patients suffering from acute disease, Dr. Anstie presents four cases observed by himself, in all of which, from circumstances, *no other substance but alcohol and water* could be taken. In the case of a child aged fourteen months, port wine was the sole nutriment for twelve days. In the case of a man aged eighteen, there was total abstinence from all but alcohol for seven days, and but little ordinary food was taken for another week. In the case of a man aged twenty-four, nothing but alcohol and water was taken for ten days. In the case of a man aged seventy-six, nothing but alcohol and water was taken for six days. All these patients recovered most rapidly their full strength, and were never notably emaciated.

8. Taking the cases of disease along with the cases of health in which life is thus singularly preserved by alcohol alone, it is impossible to suppose that this substance produces these effects in virtue of any narcotic influence. The active lives of the one set of individuals, and the rapid recovery out of the depression of severe acute disease by the other, make this supposition altogether untenable.

9. We are compelled therefore to believe, that *in doses proportioned to the needs of the system at the time*, alcohol acts as a food; and that its so-called *calmative* action in acute diseases is due to a food-action, and not to a true narcotic influence. That in doses beyond this amount, whether in disease or health, it acts as a narcotic poison simply. That in moderate doses it is no more an "anæsthetic" than is chloroform in small doses. That in large doses it is simply a narcotic poison of the anæsthetic class, and that the so-called stage of "depressive reaction" is merely the period of most profound anæsthesia.

Is Alcohol Food?

By Dr. THOMAS INMAN, Liverpool.

(British Medical Journal, October 4th, 1862.)

In a paper read before the British Medical Association, August, 1862, Dr. Inman states the following propositions:—

1. Nature has provided in the salivary glands, the liver, and the lungs of every mammal an apparatus for converting all food, especially farinaceous, into alcohol; and we have no evidence that such conversion does not take place. Chemistry has in vain hitherto attempted to explain the source of the whole of the carbonic acid formed in the blood and exhaled by the skin and lungs, for it has as yet ignored the fact that the lungs, amongst their other functions, may be the fermenting vats of the animal economy. It is true that alcohol has not yet been demonstrated to exist in the arterial blood of animals; but it has not yet been looked for. Saccharine and farinaceous matters only require air and water to change them into alcohol and carbonic acid.

2. One form or other of alcohol is available for the support of life, and for restoration to health when no ordinary food can be taken. This fact is very conspicuous in cases of fever, hæmorrhage, and infantile dyspepsia.

3. Alcohol, after being taken, is incorporated with the blood, passes into the various tissues, and ultimately disappears—a small portion only passing away in the breath. We can say no more of bread, potatoes, or oatmeal porridge, a small portion of each of which passes out of the body with the fæces.

4. Alcohol, in the form of ale, porter, wine, &c., relieves hunger and quenches thirst simultaneously, and with a completeness which is not equalled by water, infusion of gentian, cayenne pepper, or by turpentine: *i.e.*, it does not act as water simply, or as a stimulant alone.

5. Wine, beer, &c., satisfy the appetite when taken alone, and act for the time like any solid food would do. Hunger is allayed; exhaustion relieved; the veins show an increased amount of blood; a genial glow evidences a more vigorous circulation; and the brain, which is an organ peculiarly sensitive to the quantity and quality of the blood circulating through it, shows by its increased power that the alcohol has entered the circulation and influenced it. After a time this effect subsides, precisely as in the case of ordinary food. A large excess in food is equally capable of causing lethargy, coma, and convulsions as a large excess in alcohol.

6. When alcohol is mingled with other food, a less amount of the latter suffices for the wants of the system than if water had been used as the drink. Dr. Inman cites his own experience of an attempt to do without his ordinary allowance of ale at dinner: a large increase of food was necessitated, but the demand for this diminished at once on resuming the ale. Similar facts were noted in the experience of various members of his family. No loss of health or

strength was experienced, except when the ordinary amount of solids was taken *without the beer*.

7. The various forms in which alcohol is taken have as marked and specific effects as have animal and vegetable articles of diet. Dr. Inman cites the following cases in point:—(a) A surgeon's widow, after the period of the birth of her son (who grew up healthy), seemed not to regain her strength, her appetite failed entirely, but she had abundance of milk. *For many weeks she took nothing but brandy-and-water.* (b) A surgeon at Wavertree attended a young man with hypertrophy and patulous valves of the heart, from September 24, 1855, to April 26, 1860. For the last five years no animal food would remain on his stomach, and farinaceous food he would seldom take. In the first two he lived principally, and for the last two years and a half entirely, on brandy-and-water; the allowance of brandy was at first 6 oz., afterwards one pint per diem. He kept good flesh and spirits nearly to the last. (c) A patient in the Royal Liverpool Infirmary, who had been severely salivated, lived for a fortnight upon beer alone, and appeared to lose no flesh. (d) Mr. Nisbet, of Egremont, knew a man who for seven months was unable to take any food, and lived solely on spirit and water. (e) The same gentleman observed the case of a child afflicted with marasmus, who lived for three months on sweet whisky-and-water alone, and recovered. (f) Mr. Nisbet also saw a case in which a child subsisted for a fortnight on Scotch ale alone, and then recovered appetite and strength. (g) A lady, 25 years of age, florid, *embonpoint*, of active habits, but delicate constitution, was a patient of Dr. Inman's. Twice in succession she nursed a child, for about twelve months, subsisting during a large part of this time on brandy and bitter ale alone: the children grew up strong and healthy. From all these facts the conclusion is irresistible, that alcohol is food.

A Remark on "Expectant Medicine."

By Dr. LATHAM.

(*British Medical Journal*, July 19, 1862.)

"But there is" (our quotation is from some wise and eloquent *general remarks on the practice of medicine*, all of which are well worthy of attentive study,) "what is called 'Expectant Medicine.' It points to a more cultivated field of practice than that which we have been just surveying. The term is significant, and implies the attitude of expectation which the mind now maintains; waiting upon its own self-experience, and still waiting in faith of (what it deems) trustworthy authority, for a more or less distant event. Now, a rare enough thing to meet with among physicians is this wisely expectant mind. It presumes a study and sound judgment of the sort and measure of evidence which the subject admits, and a decision habitually exercised upon it; not demanding more, but never content with less.

"But there is a scepticism common among us, and much fostered by the philosophical part of our training, which is hostile to this state of mind. It requires evidence foreign to the subject-matter, or more than it admits of; and so not finding it, it believes in nothing.

"Again, there is a credulity common enough among us; and this is in sympathy with the larger credulity of the world on medical subjects, and is strengthened by it. It incapacitates for all patient inquiry; and it may come to believe in everything. Now, if there be no mean between the extremes of scepticism on the one hand, and credulity on the other, there can be no safe and successful practice in this region of 'expectant medicine.' But such a mean there is, in which all good physicians meet, and communicate, and understand one another, agreeing together in that prudent, patient, hopeful faith which they all perceive, but none can define.

"Yet, when we come to test the matter fairly, how can there be any experience in this field of 'expectant medicine?' For how can the management of individual cases of most chronic diseases, whether by cure or treatment, be conformed to the idea of an experiment? Verily, to any great degree of perfection, neither one nor the other is possible. But even in 'expectant medicine' experiment would be practicable, and experience attainable in much higher degrees than they are, if physicians would only be content to work with fewer and simpler remedies.

"I have myself a reasonable amount of faith in the power of medicine over chronic diseases. I have laid up a certain sum of *experience* fairly collected (as I believe) from *experiments* which I have been making all my life. But then, all my life I have been careful about my experiments, in this respect especially, I have sought to manage my cases of chronic diseases—in other words, to work my experiments—as much as possible by single remedies. On any other terms, I do not see how it were possible that I should have any faith at all. It is a mystery to me how such prescriptions as the following for any known form of disease can end in any trustworthy experience:

Quinine	Ipecacuanha
Steel	Stramonium
Zinc	Colchicum
Valerian	Iodide of potassium
Nux vomica	

"Such complex prescriptions render the knowledge of the remedial effects of particular substances absolutely impossible. Do the prescribers impute a distinct effect of its own to each of the ingredients, and so reckon the separate instalment brought by each to the remedial mass? Or are they content to take it in the lump, and rejoice in the oneness of the effect?

"My excellent friend Dr. Chambers, as soon as he had chosen medicine for his profession, did not think it beneath his dignity to work at a great pharmaceutical chemist's, compounding medicines and making up prescriptions. Here he saw what had been carefully preserved, the autograph prescriptions of bygone physicians. And those which bore the initials of the most eminent were remark-

able for these two characteristics, their plain and legible penmanship, and the very few and simple articles which they directed. Surely, it is not unsafe to read the men's minds in these documents, and conceive the character of their thoughts and proceedings in the great business of their lives. The men were evidently candid and clear-sighted and of simple purpose; and among them were the best of their time, Dr. Heberden, Sir George Baker, the elder Mr. Warren, Dr. David Pitcairn, and Dr. Baillie. In our day, the profession of medicine needs a little gentle pressure from some such hands as these to steady it and keep it within bounds.

"A gentleman went from Scotland to consult a celebrated watering-place physician. His complaint was asthma. A scheme of diet was laid down for him, scrupulously and minutely strict; and he followed it to the letter. A mixed multitude of medicines was prescribed for him, which had an unpromising look of strife and incongruity. But he took them all bravely and obediently. And verily he had his reward. He obtained relief of his asthma. But the asthma would still return; and as often as it returned, he betook himself to his dietetic and remedial discipline, and it went away again; and so his faith was confirmed. In process of time, however, whether the diet was too austere or the medicine too nauseous, and so the flesh began to rebel, or whether a laudable curiosity set him to find out the secret of his treatment and relief, he certainly began to question the necessity of *all* the means to the end. So, on his next attack, adhering to his dietetic rules, he bravely took no physic. But the asthma abided, and would not leave him until he had recourse to his accustomed medicines. On the following attack, he set at nought his dietetic rules, and scrupulously took his physic; and the asthma passed away as usual.

"It was pretty plain that the physic-bottle contained the cure. But to which of the many ingredients did it belong? To one, or two or three, or to the hotch-potch working mysteriously together for good? In a matter which so nearly concerned him, the patient might be pardoned for laying his rash analytical hands upon the mysterious mixture. It contained, among twenty other things, a few grains of iodide of potassium. Ingredient after ingredient was deducted; and, simpler and simpler as the mixture became, it still had equal power to abate the asthma, until the iodide of potassium was deducted in its turn, and then its sovereign power was gone. Again, all the ingredients were tried, excepting only the iodide of potassium; but altogether they did not touch the asthma remedially. Finally, every other ingredient was excluded, and the iodide left alone; and alone it displayed a sovereign remedial power.

"Fortunate the man who can get rid of an asthmatic attack on any terms; but unfortunate the art that is content with a rare fortuitous and unaccountable success; it must be either retrograde or stationary. To scatter above twenty remedies, and to let hit which may, is like pigeon-shooting in companies. The bird falls; but whose gun was it that brought it down? Nobody is reputed the better marksman after a hundred volleys.

"With all the credit due to pharmaceutical chemistry, and all our

obligations to it, I doubt whether, in one chief respect, it has not done some harm. To bring many important remedies together, and unite them by a lucky combination, and compress them within a small compass, and so place them within the common reach, all this gives a facility of prescribing, which is hurtful to the advance of medical experience. The facility of prescribing is a temptation to prescribe; and under this temptation there is a lavish expenditure continually going on of important remedies in the mass, of which the prescribers have made no sufficient experiment in detail. A simple implement or two, which a man has well proved for himself, is worth a whole armoury of famous compounds taken upon the general credit. A few thousand years ago, a whole people was in fear and trembling. Their enemy was at the gate. Their hope was turned to a single champion. All weapons of war were at his service. The king's own armour was offered him—his helmet, his coat of mail, his sword. He did not (how could he?) resist the vanity of putting them on. But soon he put them off again; for 'he had not proved them.' And 'he chose him five smooth stones out of the brook,' and with one of these he did the deed which saved his country."

On the Goat's-Whey Cure.

By Dr. CHARLES A. LEE.

(*American Medical Times*, Feb. 28, 1863.)

It is now about sixty years since the *goat-whey treatment* came into vogue in Switzerland, in consequence, it is said, of the recovery of some high personage under its use, who had been given over by the profession. This was at Gais, one of the highest of the Appenzell Alps. Gais is the most celebrated place for this mode of practice at the present time. There are but three other villages in all Switzerland more elevated, it being over 3000 feet above the level of the sea. There are quite a number of hotels and boarding-houses for the accommodation of patients, and many lodge in private houses. Much credit is given to the vivifying effect of the air, which is dry, very much rarefied, and remarkably pure. The inhabitants have such a dread of vitiated air that they do not till the soil, but leave it in pasturage, for fear that some injurious emanations may escape from disturbing it. There are several other establishments within the radius of a few miles, all celebrated for this mode of treatment, viz., Gonthen, Urnasch, Heiden, Heinrichsbad, and Weissbad. Those who require a less rarefied air generally resort to the latter, which is situated in a narrow, deep valley, surrounded on every side by high mountains. There is also an establishment at Horn, near Rorschach, quite celebrated, which is supplied with warm fresh whey every morning from the Appenzell Alps. From this place the tourist enjoys a magnificent view of the Lake of Constance. During the day the goats climb to the very summit of the mountains, browsing

upon the plants which grow up to the foot of the glaciers, and also upon the resinous leaves and twigs which fall from the fir-trees. At six o'clock they are driven to the village to be milked, when the milk is at once subjected to the process of separating the caseine. This is generally done as follows:—The milk is turned into a large copper cauldron, suspended over the hearth on a moveable crane. When the temperature reaches about 86 Fahr. it is removed from the fire, and a little rennet added, agitating it constantly. As soon as coagulation takes place the caseine is separated by the hand or a branch of fir, in order to reduce it to a pulpy mass, when the fluid is again placed over the fire. This process is several times repeated, until all the caseine is precipitated to the bottom of the vessel, which requires about two hours. It is then elevated on a sieve, and being placed in moulds is subjected to pressure, till all the serum is separated, when it is afterwards subjected to other manipulations. The porters immediately fill their casks with the whey, and, swinging them over their shoulders, carry it, while yet warm, to the different establishments where it is wanted.

Goat's whey thus prepared has a greenish tint, and is somewhat opaque or milky from the small particles of caseine which have not been entirely separated by the process. It has a sweetish, balsamic taste, and an agreeable flavour. In fact, its sensible properties are very different from the goat's whey which is offered for sale in London and Paris, and there can be no doubt that its therapeutical effects also differ in a considerable degree. Its chemical composition may be said to consist of a solution of sugar, lactic acid, and an animal extractive matter, made up of osmazone and different salts. The predominant element would seem to be sugar. The relative proportion of caseine and sugar differs considerably, as is well known, in the milk of different species of animals. The salts consist of the chlorides of potassium and sodium, sulphate of soda, and the phosphate or carbonate of lime. Ass's milk contains a less proportion of salts than any other; while, on the contrary, goat's milk has a larger quantity of sugar. The whey is generally drunk warm, between six and eight o'clock in the morning. It is taken pure, and to the extent of seven or eight ordinary sized tumblerfuls. The patients are directed to walk about for several minutes between each cup. This is believed to aid in its digestion and assimilation, and more speedily to bring on the other desired results. Between the third and fourth cup a serous diarrhoea is said to come on, accompanied with borborygmi, without colic or tenesmus, and an hour after the last dose it is usually terminated. The patient then swallows some farinaceous soup, to counteract any further laxative effect. It is very rare that there are any more alvine discharges for the next twenty-four hours. After a few days the tongue becomes white and the mouth clammy, and there is more or less constipation, for which a mixture of equal parts of sugar, rhubarb, and cream of tartar is given, which acts as a gentle laxative. Some of the patients also take baths of goat's and cow's milk, which are said to have a very beneficial effect in cases where the skin is hot and dry, the pulse frequent, and the nervous system irritable. Physicians who follow

this specialty speak very enthusiastically of the great cures effected by the treatment, and explain the effects from the influence exerted over the secretions and excretions, and the corresponding modification of the humours or animal fluids. In scrofulous affections, of children especially, the effects are said to be wonderful. They tell us that there exists the same difference in the blood of scrofulous and healthy persons, as there is in *colostrum* or newly secreted milk and that when fully matured, and that goat's whey restores to the blood the globules or constituents which it lacks. This seems to be very probable, at least, as we know that the strength or feebleness of the body either depends very much on the composition of the blood, or bears a certain relation to the elements entering into it. There are two classes of disease for which this treatment is specially recommended, viz., those of the chest and bowels. By far the largest proportion of cases that resort to these establishments are those of bronchitis, chronic laryngitis, bronchial catarrh, and tubercular affections. In a large majority the symptoms are said to be very speedily modified, as the cough, expectorations, dyspnoea, and night-sweats; which are either diminished or totally cease, unless there is extensive organic pulmonic disease. But there will be always this difficulty in forming a just appreciation of the value of this treatment in these Alpine establishments, viz., to discriminate between the special and direct action of the goat's whey and the atmospheric and climatic influences. Both, doubtless, have their effect; and when we consider that both incipient goitre and cretinism, if not scrofula, may be arrested, and even cured, by removing the patient from the low valleys to high elevations among the Alps, we shall hardly be willing to admit that the goat's whey plays the most important rôle in these cases. In habitual torpor and constipation of the bowels there can be little doubt but that this special treatment effects good results. The immediate contact of this bland, mildly stimulating fluid upon the mucous membrane of the intestinal canal produces a constant depurating effect, which tends to disgorge the viscera and favourably modify their activity. It is the uniform testimony of the patients, moreover, that one constant effect of the treatment is to increase the appetite and favour nutrition. The duration of this special treatment averages probably three or four weeks, though in many cases it is much longer. It is not uncommon at the mineral springs to associate the use of the goat's whey with that of the waters, especially here in Germany. The custom is, for several days to mix equal parts of whey and mineral water together, and afterwards one-third or one-fourth, till, at last, the whey is taken pure, and is continued for a longer or shorter time, according to circumstances. The commencement of summer is considered the most favourable time to begin this treatment. The milk is most abundant in the spring, but then it is less sapid, as the sun has not yet sufficiently developed in plants the juices and peculiar proximate principles which impart to milk its therapeutic properties. Much importance is very justly attached to the locality where this treatment is to be tried. All must admit that the quality of the whey must depend, of course, on that of the milk; and milk cannot

be healthy and pure where the nourishment and mode of life are not conformable to the nature of the animal. Thus, cows and goats which are confined in close stables and in large cities, necessarily deprived of pure air and fed on unnatural food, die, for the most part, of tubercular disease. We can certainly draw no safe conclusions from the use of whey obtained from the unhealthy milk of such animals, for it has no analogy with that derived from those which range and browse on the mountains of Switzerland. The establishment at Appenzell is generally preferred to the others above named, for the reasons already stated, although good results are observed at the Righi, at Interlaken, Kreutz, Weissenstein, &c., &c. The goat's-whey establishments of Germany, not being so favourably situated, do not enjoy so high a reputation as those of Switzerland. The most important are those of Baden-Baden, Badenweiler, Rehburg, Rosenau, Schlangenbad, Gleisweiler, Ischl, Neuhaus, Gleichenberg, Minden, Botzen, and Meron in the Tyrol. The last is especially celebrated.

On the Lithia Springs of Baden-Baden.

By Dr. ALTHAUS.

(*Medical Times and Gazette, and Pharmaceutical Journal, May, 1863.*)

The salts of lithia, into the pharmaceutical properties of which Lipowitz and Mr. Alexander Ure were the first to inquire, have recently been prominently brought under the notice of the medical profession by Dr. Garrod, who recommends them in cases of uric acid diathesis connected with gravel, and in cases of chronic gout. This recommendation is based upon the fact that lithia possesses a great affinity for uric acid, and that the urate of lithia is the most soluble of all the urates.

Lithia is especially found in the lepidolite, a kind of mica which occurs in the granite of Bohemia, and from which it is prepared in chemical manufactories. From one of these fabrics, about forty pounds of carbonate of lithia are annually sent to this country, where the salts of lithia are now so much in demand that they can scarcely be supplied in sufficient quantities. The amount of lithia which had been found in mineral waters up to the present time was very trifling; and it was the great rarity of this substance which prevented Mr. Ure from following up his researches on it. Quite recently, however, Professor Bunsen, of Heidelberg, has shown, by a new method of investigation, that lithia is, indeed, a substance which is most universally encountered in Nature. It is well known that many substances, when introduced into a flame, have the property of imparting into the spectrum of the flame peculiar and brilliant rays, which are especially striking when the heat of the flame is very great and its light insignificant. Upon this fact the Professor of Heidelberg has based a method of qualitative chemical analysis, which has considerably enlarged the domain of chemical

investigations, and has allowed to solve problems which were hitherto believed to be unapproachable. By means of this method, Professor Bunsen discovered in the mineral springs of Dürkheim two new metals, to which he has given the names of *cæsium* and *rubidium*, which are closely related to potassium, and are the most electro-positive substances known. Concerning lithia, the same philosopher found that its compounds give rise to two quite distinct rays, one of which is of a feeble yellow and the other of a very brilliant red. By this method $\frac{1}{100000}$ of a milligramme of carbonate of lithia may be distinctly traced, and one-fourth of a grain of the same salt causes the red ray to continue for a whole hour. The nature of the compound in which the metal exists has no influence upon the position of the rays in the spectrum, it being the same with the chloride, the bromide, the iodide, the carbonate, the sulphate, the phosphate, and the silicate of lithia; but the intensity of the rays increases in proportion to the volatility of the compounds. Professor Bunsen has by this means discovered lithia, not only in triphylite, triphane, and petalite, but also in a large number of feldspaths, in the granite of the Odenwald, in a common spring at Schlierbach near Heidelberg, in sea-water, in fucus which had been carried by the Gulf-stream to the shores of Scotland, in the ashes of tobacco, of grapes, of vine-leaves, of grain of every description, of the milk of animals which had been fed upon grain, &c. Lithia has also been discovered in the ashes of the human blood and muscles. A very large quantity of it was found in two of the thermal springs of Baden-Baden, namely, the Fettquelle and the Murquelle, of which the former contains 0.2315 grains of chloride of lithia in sixteen ounces of water, and the latter 2.3649 grains of it. In one hundred pounds of the salt extracted from the Murquelle nine and three-quarter pounds of lithia are contained; that is, a quantity of this substance worth 90% sterling. This amount is not equalled by that contained in any other mineral spring which has yet been examined.

In consequence of the analysis of these springs, made by Professor Bunsen, they have during the last season, for the first time, been extensively used in cases of gout and lithiasis; and Dr. Althaus is indebted to his friend, Dr. Reuf, of Baden-Baden, who has treated a large number of cases of that description with them, for the following particulars regarding the result of their administration.

The physiological effects observed after taking the waters are as follows:—At first they promote digestion, and a feeling of well-being is induced; but after they have been taken for some time, and especially in large doses, sickness, disposition to vomiting, and diarrhoea ensue, which in most cases, however, gradually disappear, but sometimes continue as long as the water is drunk. A constant effect is an increased elimination of urine, the quantity of which is often doubled, or even trebled; it becomes turbid after some time, and large quantities of a reddish sediment are deposited in it. In some of the patients treated by Dr. Reuf, profuse perspiration came on after from five to ten days, and continued as long as the water was drunk; in the case of a lady who had not freely perspired for years, this perspiration even continued two months after the cure

had been finished. It therefore appears that the water is a diuretic as well as a diaphoretic.

Concerning its therapeutic action, in almost all cases the pain in the joints increased at first (especially in those patients who were in the end cured) to a rather high degree, but it never spread to healthy parts. In joints which were perfectly contracted, crackling, dragging, and pulling were felt, as if the articulation were being torn asunder; but after such an attack of pain, a sensation of easiness and decided improvement was felt, and the mobility of the limb was much increased. In one patient, a physician from Epernay, a regular fit of gout came on during the use of the water, under the continued use of which this patient so rapidly improved, that he could walk about again after three days.

Gouty affections of the joints, of the sheaths of the nerves and the muscles, if not of very long standing, were cured after three or four weeks, and have remained so up to the present moment. In periodically recurring headache on one side, which is often due to gout, the effects were also very beneficial. A lady who had been contracted for fourteen years, and who could neither stand, nor walk, nor carry a spoon to the mouth, was by the use of this water so much improved that she was able to walk a little, and to stand and eat by herself, while no former medication had relieved her. This patient also suffered from dysmenorrhœa, the most prominent symptoms of which were severe abdominal pain, oppression and asthma, cold and paralysis of the left arm. She had only taken the lithia water for eight days when the catamenia appeared, and were unaccompanied by any unpleasant symptoms whatever; nor did these latter reappear afterwards. In a male patient whose finger-joints were infiltrated with urate of soda, which was visible in white specks through the skin, these infiltrations were removed, and the swelling diminished.

The mode of administering the water was as follows:—For patients with whom large quantities of water do not agree, five grains of the carbonate of lithia were added to a bottle of the water of Murquelle, which contains five grains of chloride of lithia; and the water was then impregnated with carbonic acid, in order to render the carbonate more soluble. Of this water a tumblerful was drunk three times a day; and if an increase of the dose appeared necessary, two or three grains of the carbonate were added to every glass. If patients are able to keep much water on their stomach, they may take six or eight tumblerfuls of the Murquelle, without any artificial addition of carbonate of lithia. Baths with water of the same spring were also given; and the Administration of the Spas of Baden-Baden are now occupied in preparing a motherlye from the water of the Murquelle, in order to gain so much of the salt that it may next year be added to the baths. Of all the antarthritic remedies offered by the springs of Baden-Baden none have proved so beneficial as this lithia spring. Dr. Reuf has, in some cases, at the same time employed the Russian vapour-baths prepared from the steam of the hottest spring of the place, which has a temperature of 155° Fahr.

On the Influence of Sleep over Disease.

By Dr. WARE.

(Boston Medical and Surgical Journal, January 16, 1863.)

In a lecture on *General Therapeutics* Dr. Ware, after reprobating the practice which some medical men have of having their patients awakened at short intervals in order to give wine or food or medicine, makes the following remarks upon the influence of sleep over disease. He says:—

“In all forms and conditions of disease, both acute and chronic, the state of the patient as to *sleep* is an important consideration, both as regards his comfort and also as regards the satisfactory progress of his case. The nature of this condition of animal life we do not fully understand; we only know that it is a necessary one, and having a vast influence on the state of the system. Its purpose seems to be to afford an opportunity, by the suspension of certain activities of the system, which require the exhaustion of those powers that emanate from the nervous system, for the reinforcement of those powers. *It is also during sleep that the repair of the tissues by nutrition is provided for.* Not that all nutrition is suspended during our waking hours, or that all waste is suspended during sleep; but that in two states of sleeping and waking there is respectively a large predominance of the repair and the waste. Sleep is not solely rest, as it has been sometimes considered, an entire rest of all the organs at once; it is something specifically different. It is a condition of an entirely different nature, and a condition for which rest is not, in any sense, a substitute. The mere facts of existence, without exercise, without fatigue—the simple going on of life—implies a certain expenditure of force which renders necessary, at certain intervals, a suspension of those functions of the brain and nervous system which are subservient to the phenomena of mind. It is possible that ordinary rest might afford an opportunity for the nutrition of all these tissues, except those which are the agents of the mind. But it seems to be necessary, for the repair of these, that the functions of the mind should also be suspended. Of the physical condition of the brain in sleep, and also concerning the peculiar state of the mind in sleep, notwithstanding the many theories which have been formed concerning them, we know nothing with certainty; and this is not necessary to the practical management of the sick. What should guide us is the knowledge that a certain amount of sleep, at proper intervals, is an absolute necessity, and that its absence or its deficiency is always a great evil, and to be prevented by every possible means. In acute diseases a sufficient amount of quiet sleep is at once a favourable indication of the nature and issue of a case, and also is an important agent in the promotion of a favourable issue. Its absence, on the contrary, is, *pro tanto*, an unfavourable indication as to the result, and also promotes an unfavourable issue. Want of sleep adds to the sufferings of the patient, and also to his exhaustion, and consequently

interferes with the success of the sanitary process, and impairs the power of recovery. In every point of view, then, the state of the patient in this respect becomes the object of special attention. Salutory changes in the condition of a patient will be often found to take place during sleep, and to manifest themselves most obviously on awaking from that which has been sound and refreshing."

On the Difference of Behaviour which Certain Varieties of Starch exhibited when Treated with Salivary Diastase in Saliva and in the shape of Infusions and Mixtures of Salivary Gland Substance.

By Dr. ROLLESTON, Linacre Professor at Oxford.

(*Dublin Medical Press*, Oct. 15, 1862.)

At the last meeting of the British Association for the Advancement of Science at Cambridge, Professor Rolleston read a paper on this subject, of which one result appears to be that starch foods are useless in early infancy. The chief results arrived at are these:—

1. Inulin from the dahlia retains sugar with great tenacity, but, by repeated washing, it can be freed from this impurity.

2. When thus freed from sugar, it obstinately resists the converting influence of salivary diastase.

3. This salivary diastase was obtained from human saliva, and from parotid and submaxillary gland substance infused with water and buccal mucus.

4. The same salivary diastase instantly converted ordinary starch into grape sugar.

5. This salivary gland infusion, however, if made with salivary gland substance from young animals yet sucking, Dr. Rolleston had found to be ineffectual upon ordinary starch. Bidder's researches were in accordance with his.

These results led to the two following practical rules:—1. Artichokes are little likely to act as a substitute for the potato, as they contain inulin *vice* starch. 2. Starch foods are useless in the early months of infancy, as salivary diastase at such a period is inactive.

On Rennet Wine.

By Dr. GEORGE ELLIS.

(*Dublin Medical Press*, July 16, 1862.)

Dr. Ellis believes that rennet wine will do all that pepsine is said to do—that is, very much more than what pepsine is really able to; and he appears to have very good reason for his belief. He says:—

"About two years since, failing to obtain benefit from pepsine, I had recourse to the direct preparation of a solution of gastric juice

from the calf's stomach; and I have found the result so gratifying, its effect in gastric derangements so satisfactory and remarkable, both in my own hands and in those of several medical friends to whom I recommended it, that I wish to communicate to the profession, more extensively, the following mode of preparation which, after many trials, appears to me to be the simplest and most convenient for general prescribing purposes. Take the stomach, or rennet bag, as it is called, of a calf *fresh* from the butcher; cut off about three inches of the upper or cardiac extremity, which portion, as it contains fewer glandular follicles, may be thrown away; slit up the stomach longitudinally; wipe it gently with a dry napkin, taking care to remove as little of the clean mucus as possible; then cut it into small pieces (the smaller the better), and put all into a common wine bottle; fill up the bottle with good sherry, and let it remain corked for three weeks. At the end of this time it is fit for use.

Dose.—One teaspoonful in a wineglassful of water immediately after meals.

Test of Quality.—One teaspoonful will solidify, to the consistency of blancmange, in from one to two minutes, a cup of milk (about eight ounces) at the temperature of 100° Fahr.

"In this action on the casein of the milk, it may be said that the wine itself might have some effect. This, however, cannot be the case, as wine will not solidify milk, and it will only curdle it at a much higher temperature and in larger proportion.

"This preparation, which I propose to call 'rennet wine,' has many advantages over the watery infusion of rennet which is obtained from the dried and salted calf's stomach (used largely in cheese-making). The objections to the latter are, that it is much more troublesome to prepare, and becomes very soon spoiled in warm weather when it begins to react on the animal matter contained in it. Rennet wine, on the contrary, is so easily made, requiring no drying or salting of the stomach, is so inexpensive, and can be so easily prescribed in private and hospital practice, that I have little doubt if known and tried it would become a very highly valued remedial article in the hands of the profession, and would take a permanent place on the shelves of the apothecary.

"I recommend the employment of good sherry, because this wine has sufficient body to keep the infusion sound for any length of time, and is not so strong in alcohol as to interfere with its power of taking up the active principle of the rennet.

"To the physiologist it is unnecessary to say that it should be given *after* or *during*, and not before meals. A single dose given daily after dinner will be found quite sufficient to act speedily and effectively, without other treatment, in the common run of cases of functional disorder of the stomach. It is not, perhaps, easy to explain the operation of this small quantity when we consider the large supply of the gastric secretion required for the thorough digestion of an ordinary meal. The action is probably due to those indirect chemical changes called catalytic transformations, which some organic substances, by their mere presence and contact, induce in each other and in other proximate principles. Thus the conversion

of a small portion of food in the stomach into healthy albuminose by this small quantity of sound gastric juice may induce the same healthy action throughout the stomach contents during the entire process of stomach digestion. It is at least equally difficult to explain the action and rapid extension of ferments generally in their appropriate solutions. I have often been forcibly struck by the magical effect of this small dose in removing offensive odour from the breath of young persons—a distressing symptom sometimes aggravated rather than relieved by purgative medicine; and I may also mention that in one of these cases cod-liver oil was easily tolerated afterwards though never before.

“It would be a mistake, however, to suppose that oil is at all acted upon by the gastric juice. The oil globules of coagulated milk are seen, under the microscope, unchanged though embedded in the solidified casein, the digestion of oil being entirely intestinal. But intestinal digestion itself must surely be influenced essentially by the healthy preparatory action of the stomach secretion on the albuminous compounds presented to it, and thus the digestion of oils and fatty matters, though not commenced in the stomach, will be indirectly facilitated by their being mingled with the products of the healthy gastric operation, when submitted subsequently to the action of the pancreas and liver.”

On the Physiological Effects of Hachish.

By M. DE LUCCA.

(*Journal of Pract. Med. and Surgery*; and *Med. Circular*, Dec. 3, 1862.)

In an interesting paper, read recently at the Paris Academy of Sciences, M. De Lucca gives an account of his personal experience of the effects of a confection containing hachish, which he had received from some friend in the East. At 8 A.M., M. De Lucca swallowed from thirty to forty grains of the paste, and calmly awaited the result. He shortly after proceeded to the chemical laboratory of the College of France, and addressed himself to his habitual occupations. In the course of a quarter of an hour, he experienced at the tips of his fingers an indescribable creeping sensation, which appeared to ascend towards the brain. His mind, however, remained perfectly clear, but his hands yielding to a peculiar nervous influence, lost the power of adapting themselves to the performance of delicate operations requiring steadiness or precision. He then determined on returning home, when further phantasms were induced, without any coincident confusion of intellect.

Distances seemed unceasingly to extend, and he fancied he would never reach his house. The voices of persons standing at his side sounded weak and remote. He felt himself unaccountably elated, and as if treading the air. The porter's wife, as he passed, remarked: “M. De Lucca's rooms are not ready;” and he was

much struck with the altered character of the woman's voice. When he reached his room, he attempted, but unsuccessfully, to read his letters, and yet his mind was crowded with bright ideas, endowed with singular distinctness and precision. His entire being was given up to pleasing sensations. He retired to bed, and the sheets seemed out of respect to recede from his person, and although he fancied himself in no sort of contact with the bed-clothes, the situation was delightfully comfortable. "At that moment," says he, "I reviewed with the most intense satisfaction every action of my life, but my ideas flitted through my brain in such rapid succession, that I was unable to grasp steadily any one thought. I remarked to myself, if this condition could but last, the dreams of the poets would surely be realized, universal happiness would follow, and all men might joyfully contemplate their mental operations."

This state of inebriation lasted about four hours; the distances than decreased, the bed-clothes courteously drew nearer, the nervous excitement subsided, and order was restored; dryness of the lips was the only other symptom noted by the experimentalist.

*Further Researches on the Therapeutic Properties of
Peroxide of Hydrogen.*

By Dr. B. W. RICHARDSON.

(*Medical Times and Gazette*, March 29, 1862.)

After repeated and long-continued experiments in reference to the different processes for making the solution, Dr. Richardson is of opinion that no plan is so good as the one originally invented by Thénard, in which the peroxide of barium is used as the agent for supplying the oxygen, with hydrochloric acid as the displacing body. A solution charged with ten volumes of oxygen appears to be the best and most applicable form of administration. The dose of this solution for an adult is from one drachm to half an ounce in a liberal quantity of water. As a general rule the solution should be given separately, or if admixed with another remedy, should be admixed at the time of administration. Dr. Richardson has used the remedy now in 223 instances; viz., in simple diabetes, 3 cases; in diabetes complicated with phthisis, 2 cases; in chronic rheumatism, 1 case; in sub-acute rheumatism, the continuation of an acute attack, 2 cases; in mitral disease, with great pulmonary congestion, 4 cases; in irregularity of the heart, with cardiac apnoea, 3 cases; in struma, with formation of purulent matter constantly recurring, 1 case; in mesenteric disease, 1 case; in simple jaundice, one case; in jaundice complicated with cardiac and hepatic disease and ascites, 1 case; in cancer affecting glands of neck, 1 case; in pertussis, 9 cases; in chronic bronchitis, 9 cases; in bronchitis complicated with mesenteric disease, 1 case; in chronic laryngitis, 3 cases; in anæmia, 44 cases; in phthisis, first stage, 66 cases; phthisis, in second stage, 31 cases; phthisis, in third stage, 13 cases; phthisis (first stage),

complicated with bronchial disease, 6 cases; phthisis, in second stage, with bronchial disease, 3 cases; phthisis, together with valvular disease of heart, 2 cases; and also in a few cases of dyspepsia. Analysing these cases, the author comes to the following conclusions: That in the treatment of diabetes, the peroxide, while it reduces the specific gravity of the renal secretion, increases the quantity; so that its value in this disease is inappreciable; in chronic and sub-acute rheumatism it is of great value; in valvular disease of the heart, with pulmonary congestion, it largely relieves the attendant apnoea; in struma it removes glandular swellings, like iodine; in mesenteric disease it improves the digestion and favours the tolerance of cod-liver oil and iron; in jaundice it exercises an excellent office by improving the digestion and causing a free secretion; in cancer it seems to exert no influence; in pertussis its value is very remarkable: it cuts short the paroxysms and removes the disorder altogether quicker than any other remedy except change of air; in old-standing bronchitis, during attacks of suffocative dyspnoea, it affords rapid relief; in chronic laryngitis its caustic character renders its administration painful; in anæmia, while it exerts no specific influence *per se*, yet, combined with iron, it increases the activity of that drug; in phthisis pulmonalis in the first stages it greatly improves digestion, and increases the activity of iron; while in the last stages it affords an unquestionable and wonderful relief to the breathlessness and oppression; acting, in fact, like an opiate, without narcotism. After describing the use of the peroxide in dyspepsia and epilepsy, the author deals finally with the anomalous symptoms excited by the solution, pointing out the singular fact that in some instances where it has been pushed freely it has produced profuse salivation. That chlorine and iodine have in these effects an analogy to salts of mercury, is a fact long recognised; but the fact that oxygen in the active state exerts the same physiological action is a fact as remarkable as it is interesting. It opens an entirely new field of inquiry; it suggests the possibility that the salts of mercury do not act by virtue of the mercury as mercury at all, but by the agency of the oxygen, chlorine, or iodine, which they convey into the organism. It suggests also the propriety of ascertaining whether chlorine or peroxide of hydrogen might not replace mercury in cases where it is supposed to be a specific. If this suggestion were carried out, and an affirmative supplied, the method of cure in the disorders specified would be rendered much more simple and rational.

On Phlorydzine.

By Dr. DE RICCI.

(*Dublin Quarterly Journal of Medical Science*, August, 1862.)

This neutral principle, which exists in considerable quantities in the bark of the root of the apple, plum, and cherry tree, is recom-

mended by Dr. De Ricci as being tolerated where neither quinine nor salicine, nor bark can be administered without bringing on serious nervous excitement. The cases in which he has employed this article with most success are those of atonic dyspepsia in delicate females. He has also found it extremely well adapted to the treatment of young delicate children, or when recovering from hooping-cough, infantile fever, &c. He has given it in these cases combined with syrup of phosphate of iron and manganese, and with syrup of iodide of iron. He gives it in doses of five grains three or four times a day for adults, and proportionally smaller ones for children.

He recommends a trial of this remedy "in every adult case where quinia is not easily tolerated, as also in every case where young children require a tonic treatment either in consequence of constitutional debility, or from the debilitating effects of some previous illness; it is much more easily taken than either bark, quinia, or salicine, the bitter being of an agreeable kind, and changing, as I said above, into a sweetish taste, with the flavour of apples. I have never known it to disagree, even in large doses of 10 grains three or four times a day; and I have, in very many instances, found it of great use where other tonic substances could not be taken.

"In prescribing phlorydine it must be borne in mind that it is almost insoluble in cold water; but the addition of a very small quantity of ammonia instantly dissolves it; thus, by adding to an eight ounce mixture, containing a drachm of phlorydine, a few drachms of aromatic spirit of ammonia, the fluid, which previously was milky, becomes perfectly clear; and the addition of the aromatic spirit rather improves the mixture than otherwise.

"If a small quantity of phlorydine be previously added to the water its solving power is increased, and the mixture will be of a beautiful blue colour, but it will not dissolve as much phlorydine as when aromatic spirit of ammonia is employed."

On the Action and Uses of Codeia.

By Dr. ARAN, Physician to the Hôpital St. Antoine, Paris.

(*Edinburgh Med. Journal*, Sept. 1862.)

Writing to M. Berthé, M. Aran says :—

"I send you at last the information you ask on the experiments I have lately made with the codeia. I have as yet employed this alkaloid only to obtain calm and sleep, but from the ten or twelve cases I have witnessed I have been able to discover in this agent sedative and narcotic properties, which to my estimation place it in the first rank amongst the best remedies of this kind existing. To recapitulate in a few words the impression this medicament has made upon me, I will tell you that the codeia seems to me to contain the most marvellous and efficacious properties of opium. Inferior to morphia for calming pains, for this reason only, that it must

be given in larger doses to patients ; it has, however, over morphia a marked superiority in that respect, that it never occasions a heavy and agitated sleep ; that it does not bring on perspiration or eruptions of the skin, nor trouble the digestion ; that it produces no obstinate constipation, no desire to vomit nor any vomiting, For all these considerations codeia appears to me to be destined to become of great service in the nervous diseases of the stomach, and I can tell you that we have obtained with it some calm in cases of gastrodynia which had defied all other means, belladonna included.

"But it is especially as a means of procuring calm and restoring sleep that the codeia seems to me called to occupy an important place in therapeutics. Those stubborn and harassing coughs of bronchitis, and particularly of consumption, those violent pains of rheumatism, gout, and the organic affections, of cancer, for instance, which disturb the sleep, and frequently deprive the patients of the least moment of repose, are all forgotten in the midst of the calm and agreeable sleep which codeia procures.

"I have witnessed two very conclusive cases of incurable cancerous tumours, for which no means of relief, at all lasting, had been found. One of these tumours, of an enormous size, almost filled the pelvis, and produced on the passage of the sciatic nerve pains returning at about 8 o'clock P.M., and with such an intensity as to force cries from the patient, who could only become calm but towards the morning, when she would fall asleep, overcome with fatigue. Two centigrammes* of codeia produced the first day a calm so complete that the patient thought herself cured, and for the first time since a month she was able to take a somewhat copious meal, her appetite having returned for the first time. During the twelve days I attended this person, the pains were almost null, and as soon as they reappeared the invalid mastered them with a few centigrammes of codeia ; it became, however, necessary to increase the dose by degrees, and from two centigrammes the patient increased the dose to ten and twelve centigrammes.

"To explain how so small a dose produced a calm so complete, I must observe that her weak state rendered her very sensitive to the action of codeia, which, on the other hand, appears to me quite able to be given from the first in a much larger dose than morphia. For example, we have seen the dose of five centigrammes of it in the syrup calming the pains of gastrodynia without leaving the slightest trace of narcotism ; and one of our patients takes at present every day fifteen centigrammes of codeia without any bad effect. This invalid, who has long made use of opium and morphia, establishes between those agents and codeia a difference quite to the advantage of the latter, which does not agitate her at all.

"This, for the present, is all I can tell you. I am convinced that physicians would more frequently prescribe codeia, if the price of this medicament was not so high ; you would consequently have rendered a signal service to medical practice if your researches would permit you to lower considerably the price of codeias."

* One centigramme is equal to $\frac{1}{8}$ grain.

On the Medical and Surgical Uses of Carbolic Acid.

By Dr. BENJAMIN GODFREY, of Enfield.

(Medical Circular, Dec. 17, 1862.)

Carbolic acid is closely allied to kreosote. Its composition is $C_{12}H_8OHO$. In its crude state it is a black oily compound, resembling treacle in consistence and colour. Many disinfecting compounds, as the Liquor Carbo. Detergens, and McDougall's Patent Disinfecting Powder, owe their efficacy to its presence. Dr. Godfrey speaks in very warm terms in praise of carbolic acid as a disinfectant and deodorizer, and as a medicine in certain medical and surgical cases. For external administration he prefers the liquor carbo. detergens; for internal use he recommends purified carbolic acid, which preparation is like glycerine in colour and consistence, somewhat like unripe fruit in taste; the dose being from half a minim to two drops. This pure preparation is soluble in water in the proportion of one drachm to ten ounces. Speaking of carbolic acid, Dr. Godfrey says:—

"Its disinfecting and deodorizing powers are wonderful. In post-mortems all odour is removed. Mixed with sewerage material, at once it does its work. A few drops added to stinking urine or fetid evacuations removes all smell. As a lotion to fetid wounds, or gangrenous sores, it is highly useful, not only removing the unpleasant smell, but stimulating the capillaries to a healthy action, healing the morbid tissue. If a small quantity be put with urine fresh voided, it will keep it for many months, and undergo no change whatever. If a few drops be put into a solution of sulphate of iron, it will prevent it from oxidation. In fact, carbolic acid has a specific action upon all organic or inorganic matter, and preserves it from putrefaction or decay.

"Its surgical properties are many. Half a drachm of pure carbolic acid, mixed with half a pint of water, furnishes a capital lotion for surgical purposes. To sloughing sores, it has only to be tried, and the result will speak for itself. As a lotion to the mouth in abscess, stomatitis, and aphthæ, it is highly useful. In diphtheria and ulcerated throat, it is a boon; removing the unpleasant taste and odour, and cleansing the mucous surfaces. I have used it in cancer with very great advantage. Ten drops mixed with one ounce of zinc ointment, and applied night and morning, takes away all smell. A lotion composed of two scruples of carbolic acid and one scruple of essence of lavender, mixed with half a pint of water, and wash the feet night and morning, will remove this unpleasant condition. The same applied to the arm-pits will take away the odour of stale perspiration. It cures scabies speedily, and is preferable to sulphur ointment, because it does not irritate the skin. I have applied the liq. carbo. detergens pure to the body, and in two applications the insect is destroyed. One case I saw lately, which proved its value.

"A maid-servant had eczema upon the arms. At night the

itching was intolerable. On examination, several pustules of scabies were visible. She told me she had been under medical care for eight months. She had applied sulphur ointment and various lotions for the same. She had been taking medicine nearly the whole time, and the disease got worse. I applied zinc ointment with carbolic acid, and in a few weeks she came to me quite well.

"Carbolic acid destroys pediculi of all kinds in one application. A small portion poured into the hand, and then well rubbed into the hair, and after a quarter of an hour washed out again with soap and water, will kill every insect.

"*Its medical use* is of no less value. In all cases of stomach irritation it is of service, but especially if that derangement be produced by miasm or sewerage. The sickness of pregnancy yields to its virtue when other remedies have failed. I will mention but one case out of a great many, that will show its action.

"I was called to see a patient who was five months advanced in pregnancy. She had constant sickness, which nothing alleviated. Whether in the recumbent or upright position, she vomited. I tried nearly all the remedies of the Pharmacopœia, but in vain. Even opium would not remain upon the stomach. She at last became so reduced, that, if it could not be stopped, I must have induced premature labour to cure this frightful symptom. She was obliged to keep her bed the whole time. At last I tried the following:—Carbolic acid, three minims, water eight ounces—a sixth part every four hours. Only four doses were taken, and from that moment the sickness ceased. The first dose was with difficulty kept down, the second relieved the nausea, and by the time she had taken four doses she asked for food.

"It is of service in the flatulence of old age, depending upon imperfect digestion. I have not yet had an opportunity of trying its action upon Asiatic cholera, but I have reason to hope that it may be of great value. In diarrhœa, resulting from bad drainage, it is of service, and certainly relieves the irritation of the bowels."

On the Anæsthetic Action of Bromide of Potassium on Mucous Membranes.

By M. RIEMSLAGH.

(*Journal of Pract. Med. and Surgery*; and *Medical Circular*, Oct. 15, 1862.)

The *Archives Belges de Médecine Militaire* relates several cases of gonorrhœa, in which, after the usual remedies had proved inefficacious, M. Riemsлагh, a military surgeon, resorted with the greatest benefit to the exhibition of eight or ten ounces of tar-water daily.

The results obtained by the exclusive administration of this remedy exceeded the anticipations of the author. From the very first day, marked improvement was observable, the pain decreased, the desire to pass water became less frequent, and a smaller quantity of puriform matter was found in the urine. In the course of a week, a

complete cure was effected; no other treatment was employed, nor was any change made in the diet of the patients.

Some of the men under treatment for gonorrhœa suffered from painful erections, which yielded in a singularly prompt manner to bromide of potassium (15 to 30 grains), taken in two or three doses in the evening at intervals of one hour. The tendency to erection subsided, or at least the concomitant pain was removed.

It is a remarkable fact that the anæsthetic effects of this drug are not confined to the mucous membrane of the organs of generation, but are observable on all the mucous surfaces of the body. It has already been ascertained that common sensation is so powerfully deadened in the fauces by the use of the remedial agent, that M. Guersan and other surgeons have had recourse to it in order to facilitate the performance of staphyloraphy in children. But the same singular phenomenon may also be noticed on the mucous linings of the nose and eyes, special sensation being at the same time preserved, at least so says M. Riemsлагh. This gentleman relates a most interesting case which will doubtless lead to numerous applications, in which the most complete anæsthesia of the conjunctiva was obtained with the assistance of bromide of potassium.

The patient was a soldier of the 1st Regiment of Infantry, who had been wounded in the face by the discharge of a pistol. An immense number of particles of gunpowder had penetrated into the skin of the face and eyelids, into the conjunctiva, and even into the sclerotic membrane.

M. Riemsлагh exhibited the bromide in the evening, and next day the insensibility of the conjunctiva was so perfect, that the membrane was touched, raised, partially removed, and the gunpowder was extracted from the sclerotic itself without the least manifestation of pain.

Administered in small doses, in the course of twenty-four hours, the remedy, says M. Riemsлагh, is far less efficacious, probably because it is eliminated by the urine. Like quinine, iodide of potassium, and other drugs, the bromide of potassium should be taken in 15 to 30 grain doses in the course of an hour; its action is thus more marked, and its effects on the mucous textures more lasting.

A New Illustration of the Virtues of Transfusion of Blood as a Therapeutical Means.

By Dr. NUSSBAUM, of Munich.

(*Medical Times and Gazette*, June 7, 1862.)

The particulars of the following case are supplied by the foreign correspondent of the *Journal* whose name and number has just been mentioned:—

CASE.—This case, which occurred in the surgical clinique of Professor Nussbaum, is that of a young man of nineteen years, who, after an injury to the right knee-joint, had become affected with arthrocaecæ. The leg and the

foot being quite healthy, resection of the knee-joint was believed to be preferable to amputation of the thigh; but after the former operation had been done, the patient's condition became considerably worse. A few days afterwards he was so much exhausted that death appeared to be imminent. The pulse could no longer be felt, and the extremities were quite cold. Under these circumstances transfusion of blood seemed to be the only means of saving the patient's life. Professor Nussbaum used for this operation a simple glass syringe, set in silver, and furnished with a champagne-cork covered with doe-fur. This does not require oiling, by which impurities might perhaps be mixed up with the blood. Venesection was then performed upon several persons, whereby the professor came into possession of one pound of blood. This was beaten up for eight minutes, and then filtered through fine linen and put into a tumbler which was placed in a basin filled with hot water, until the temperature of the blood had risen to about 100°. A bandage was then put on the left arm of the patient, the cephalic vein was laid bare by an incision two centimètres long, and embraced by a strong double silken ligature. With one of the two threads the peripheral end of the vein was tied, while by the other, the central end of the vessel was raised, so that it was bent, and a small quantity of blood was confined in its canal. A semilunar incision in the vein was then made by means of a pair of scissors, and a canula and syringe, which had been previously warmed and filled with blood, were introduced. The canula and vein were then firmly tied together with the upper ligature, the bandage was taken off, and the blood slowly injected. The syringe could only hold two ounces of blood, and it was therefore necessary to remove it from the canula several times and cautiously refill it; while the latter remained upright in the vein during the whole operation, and constantly contained blood, which does not enter the vein, unless under the pressure of the syringe. Before this was again connected with the canula, distilled water was dropped into the latter, in order to prevent the entrance of air into the vein. After the whole amount of blood had been injected, Professor Nussbaum removed, first, the peripheral ligature, and afterwards the central ligature and the canula. The wound was then closed by three sutures, and an ordinary bandage, as after venesection, was put on. In this manner, not only the entrance of air, but also of coagula of fibrine into the vein was avoided, and the vessel itself only slightly injured, so that no inflammation had to be feared. During the operation the patient perceived a sensation of pleasant warmth; and a short time afterwards the pulse could again be felt, and the extremities regained their normal temperature. The patient's life was thus saved, and amputation of the thigh was, at a later period, successfully performed upon him. The ease with which transfusion may now be performed, and its wonderful and instantaneous results, should encourage us to resort to it more frequently than is done, not only in cases of acute anæmia, but also in certain chronic diseases with consequent exhaustion of the patient. Immediate transfusion, however, where the blood is made to run from the artery of one person directly into the vein of another, is connected with such inconveniences and difficulties, that surgeons will only seldom, if ever, feel called upon to undertake it.

Case in which Hypodermic Injections were used for a long time with much advantage, and without any disadvantage.

By Dr. LEVICK.

(*American Quarterly Journal of Medical Sciences*, Jan. 1863.)

This case, in which the morphia was injected hypodermically, once a day for five months, twice a day for three months, and thrice a day for six weeks, with great relief to the patient, and with no unpleasant sequelæ, is related in the Transactions of the College of Physicians of Philadelphia. It is of interest, as showing the length to which we may go with this mode of treatment.

CASE.—The leading incidents of the case are as follows: — — —, a highly intelligent gentleman, well known to many Fellows of the College, three years before suffered from an attack of myelitis, with paralysis of the lower extremities, referred to injuries received by a fall. From this illness he recovered to such an extent as to resume his usual engagements, walking to and from his place of business without difficulty.

In the early part of December, 1861, he began to experience sharp, shooting pains near the hip, following the course of the sciatic nerve, and extending to the calf of the leg. These pains, which at first were slight, gradually increased in severity, and after a few days Dr. Levick was for the first time called on to visit him. The phenomena presented were those of intense neuralgia, with the usual absence of fever or other constitutional disturbance. The previous history of the patient was such as to induce the suspicion that there might be organic disease at the root of the nerve, but at this time and later careful investigation failed to detect this, the possible existence of which was, however, never lost sight of in the subsequent treatment of the case. The pill of carbonate of iron, with small doses of extract of belladonna, and extract of nux vomica, were prescribed and taken for a fortnight or more without any benefit. As the attacks were somewhat paroxysmal, sulphate of quinia, in full anti-periodic doses, was next resorted to, but with no appreciable advantage. In fact, from this time until the early part of January, 1862, there was a steady increase of the patient's sufferings. The pain then lost much of its paroxysmal character; its intensity was such as to prevent sleep, so that the patient spent most of his nights in his chair. This loss of sleep and protracted pain seriously affected his general health; he lost flesh, and his nervous system became so enfeebled that he was unable to attend to business; even the effort to write his name, or the slightest mental exertion, being attended with an increase of suffering. Up to this time he used successively various narcotic and other remedies. The extract of belladonna, by pill and by suppository, the external and internal use of aconite, extract of hyoscyamus, morphia in large and repeated doses by the mouth, laudanum enemata (70 drops), and the sulphate of morphia in grain doses endermically, had each been tried, but unavailingly, as the discouraged patient himself said, "with little more effect than so much water." After this Dr. Levick determined to try the subcutaneous injection, and with this view introduced fifteen minims of the solution of sulphate of morphia (Magendie's solution, gr. xvj. to f3j.) into the tissue of the affected thigh. The relief afforded was prompt and decided, and the patient, for the first time for several weeks, passed a comfortable night. The pain, however, recurred next morning, and continued during the day; the injection was repeated next night, and with the same good effect. In this way it was repeated night after night, the dose gradually increased

until it reached the capacity of the syringe, twenty-eight minims. Dr. Levick says that the temporary relief thus afforded was almost magical. Frequently on his evening visit he would find his patient in an agony of suffering, and five minutes after giving the injection, would leave him composed quietly for the night.

During this time measures for the permanent relief of the patient were not neglected. In the early part of February, the injection was given twice daily, the second dose, about twenty minims, at one o'clock P.M., and, not to weary the college by unnecessary details, after this all other modes of medication were abandoned, and the hypodermic injection solely relied on. In the latter part of February, a third injection daily was given at about five o'clock in the morning, the effect of the evening's dose beginning to pass off by that time. Twenty minims were given at this time. To recapitulate, from the early part of January to the middle of June, the injection was used every night; from the early part of February to the same time in May, twice daily; and from the latter part of February to the middle of April, three times daily. During this time no unpleasant head symptoms occurred. On the contrary, a marked improvement of the nervous system was evident, and the patient gradually engaged in pursuits requiring increasing mental efforts. As this improvement took place, the early morning and the mid-day injections were gradually omitted, while that at night was continued as before. An attempt was several times made to inject in other parts of the body than the affected thigh, but this was not satisfactory to the patient, and with, perhaps, three or four exceptions, the injections were all made in the right thigh. That this mode of medicating was not a very painful one, may be inferred from the fact that it was at the earnest solicitation of the patient himself that other modes were abandoned, and that the third daily injection was given him.

As the mild weather approached, the invalid went out daily to drive, spent some days in the country, and in June went to the seaside, where, as his health improved, the remedy was entirely withdrawn. To do this was no easy matter. The relief afforded had been so great that the patient dreaded its abandonment, and this last was only effected by the cautious management of his devoted nurse, to whom its administration had for some months been entrusted. It was, however, successfully accomplished by the substitution of water, drop by drop, for the morphia solution. After a brief residence at the seaside, the patient returned to the city, gradually resumed his business engagements, and remained in good health during the summer and autumnal months. During the recent cold weather there had been some return of pain, but not such as to confine him to the house.

Dr. Levick says, that in this case no claim is made for a radical cure, by the hypodermic injection, of the disorder under which this patient laboured. But what was of interest in the case, and what was claimed for this mode of treatment, was, *First*. That after other measures had signally failed, the subcutaneous injection afforded prompt, and, for a time, complete relief from pain. *Secondly*. That by the regular repetition of the injection the patient was made comfortable, refreshing sleep obtained, and in this way life was protracted until the weather had become such as to permit him to use those hygienic measures which resulted in the restoration of his health. *Thirdly*. That although the injection was used so frequently, and for such a long time, no unpleasant sequelæ resulted. There was no disturbance of the head, and no nausea; no abscesses of the cellular tissue formed; and although the points of insertion were so numerous that it was sometimes difficult to find a spot where the instrument had not already been introduced, yet all these healed readily, and there was now nothing left to indicate where the injections had been used.

On the Excretion of Mercury during and after Mercurial Treatment.

By Dr. SCHNEIDER.

(*Schmidt's Jahrbücker*, No. 3, 1862.)

Dr. Schneider has instituted a number of experiments and investigations in order to determine the question of the excretion of mercury after mercurial treatment. He has found that the most delicate test of the presence of mercury in the liquid state is sulphuretted hydrogen; but when the mercurial preparation is contained in the urine, it is not detected so readily as in pure water. Another very delicate test of the presence of mercury is obtained by the electrolytic precipitation of the metal from its solution. By this method the smallest portion of mercury may be separated from the most dilute solutions and rendered accessible to chemical reagents. The latter are indispensable for the detection of the mercury, because the electrolytic deposit is generally not sufficiently evident to obviate the necessity for further tests. By the application of galvanism the mercury is deposited upon the cathode, when amalgamation takes place, and the presence of quicksilver may be further rendered evident by converting it into vapour and bringing it into contact with the vapour of iodine, which produces the well-known scarlet iodide of mercury.

After showing the possibility of determining the presence of mercury, by analysis, in organic fluids, Dr. Schneider proceeds to record his investigations upon the human subject. He accordingly gives the results obtained by him from the urine, first, of individuals affected with secondary syphilis, but who had never been treated by mercury; and, secondly, of some who were affected with syphilis, and who had either undergone mercurial treatment a long time before the experiments, or who were still undergoing the treatment, or who had been taking iodide of potassium after the mercurial treatment. In some other cases which terminated fatally, Dr. Schneider was able to examine not only the urine, but also the liver and the brain; and in one case pieces of bone, the liver, spleen, kidneys, and brain were all subjected to experiment.

In the urine of syphilitic patients who had never undergone any mercurial treatment, no quicksilver was detected by electrolysis; and in cases where the mercury was rubbed into the skin, no traces of the metal were found in the urine. During the internal use of the mercurial preparations, however, the urine constantly contained quicksilver. Within two years Dr. Schneider has examined fourteen cases of this kind, and has always obtained positive results. The excretion of the mercury lasts for some time after the end of the treatment, and Dr. Schneider has constantly found it in the urine a week afterwards; in one case he found it four weeks afterwards, and in another case six weeks afterwards. With regard to the effect of iodide of potassium on the mercurial treatment, his experiments do not favour the generally received opinion that the internal

use of the iodide facilitates the excretion of mercury from the system. He has made a number of experiments during the last two years in order to determine this point, and has examined the urine of persons who have taken the iodide of potassium both during and after a mercurial course. When the iodide of potassium is taken immediately after a mercurial course, the urine contains mercury, but the same is the case when the iodide is not employed. In a fatal case in which death occurred during the mercurial treatment, Dr. Schneider subjected the liver and the brain to chemical examination, and found mercury in both, but the liver contained more than the brain. In another case, in which the patient died of pericarditis some weeks after the cessation of the mercurial treatment, various parts of the body were examined chemically, but the kidneys alone gave very slight traces of mercury, and the solution obtained from the liver gave only a doubtful reaction.

On the Antiseptic Properties of Ammonia.

By Dr. B. W. RICHARDSON.

(*British Medical Journal*, May 3, 1863.)

In 1850 Dr. Richardson communicated a paper to the Medical Society of London on the antiseptic properties of gases. In 1858 he began the present inquiry on the application of ammonia as an antiseptic. His attention was then directed to the fact that the presence of ammonia effectually arrested the oxidation of various bodies, and even prevented the action of ozone. Believing that, by an extension of the same law, animal substances exposed to ammonia could be prevented from putrefaction, he kept blood and portions of tissues in contact with simple ammoniacal vapour, and with results which were most remarkable. Blood in an ordinary stoppered bottle, if charged with ammonia so as to give a faint ammoniacal odour, would retain its freshness and many of its properties for years. Animal structures in like manner placed, even so as to be massed together, in bottles containing ammonia-vapour, would retain their freshness apparently for an unlimited time.

At the meeting of the Medical Society of London, at which the paper under consideration was read, Dr. Richardson now showed the following specimens:—The lungs of a calf which he had used for lecture purposes for six months, and which had been simply placed under a bell-jar, a little ammonia in solution being put over them from time to time: a pancreas which had been kept for eighteen months in a bottle containing twenty minims of ammonia solution: a kidney showing deep congestion, which had been removed sixteen weeks: a bottle of mixed specimens, including portions of intestine with enlarged glands: a bladder, the inner surface of which was injected: an uterus and ovaries, and a pancreas, all of which had been preserved lying close in one bottle for six weeks: also a portion of liver which had been removed nearly three

years, and a cancerous breast which had been removed eight weeks before. The specimens all retained their freshness, and admitted of dissection and examination, as in the recent state.

After exhibiting these specimens, the author described the method of applying ammonia:—

It was necessary, in the first place, to trust to the ammonia alone: specimens that were exposed first to spirit, and then to ammonia vapour, were always spoiled. For the preservation of fluids such as blood or milk, it was merely necessary to add the alkali in solution, in proportion, say, of twenty minims of the strong solution to two ounces of the fluid to be preserved. For tissues, the plan was to place the specimen to be preserved in a stoppered bottle, or under a bell-jar such as is used for wax flowers and ornaments; to place in the jar with the specimen a layer of felt or lint, charged with from ten minims to a drachm of the liquor ammoniæ fortior; and then to close the vessel or jar securely from the external air. For a luting in such cases, soap answered best, or a mixture of soap and red lead.

After this description, Dr. Richardson pointed out the practical value of the method. First, in conducting *post-mortem* inquiries, it did away with all occasion for hurry. It was now only necessary at an autopsy to be provided with one or more jars, each containing, say, a drachm of liquid ammonia. The jars might now be filled with specimens; and, if the stoppers were put in with care, the specimens would retain their freshness for weeks, and even their microscopical characters. The only peculiar change was, that if much fat were present, the alkali formed with it a kind of soap—a fact which explained the formation of adipocire in the dead subject undergoing slow decomposition. For forensic purposes, this method of preserving animal structures was perfect, inasmuch as it added no mineral or poisonous matter, and yet secured the part to be examined free from change and from all offensive odour. Not only so, but important pathological changes, such as ulceration of intestine, could be kept under observation and submitted to any number of pathologists. Dr. Richardson had found the system a very useful one, too, for lecture purposes, as it enabled him to show to his class real specimens of disease, such, for instance, as the scirrhus breast, instead of casts, or specimens softened, discoloured, and, indeed, destroyed altogether, by immersion in spirit.

The last point to which Dr. Richardson drew attention was the cause of the antiseptic power of ammonia. Ammonia, being a product of decomposition, had been looked on commonly as a substance provoking decomposition. But ammonia was truly the most powerful antiseptic known: it acted catalytically, by preventing the union of oxygen with oxidizable bodies. An experiment was here performed, illustrating this. Half a grain of ammonia, diffused through forty cubic inches of air, was shown to possess the power of entirely suspending the combination of oxygen with potassium on a surface of paper saturated with iodide of potassium, starch, and solution of oxygen, so long as the paper was presented to the ammoniated air; but so soon as the paper was removed, the evidence

of the combination, indicated by the formation of the blue iodide of starch, was presented. In preserving animal structures in ammonia, the same experiment was virtually carried out; the presence of the ammonia suspends the oxidation. There were other agents which effected the purpose, such as chloroform; but the fact that these agents were indifferently soluble in water rendered them much less effective, as compared with ammonia, which combined readily with the water contained in the tissues, and so perfected the preservation of the minutest point.

In conclusion, the results tend to throw a light on the influence of the ammonias, as the producing causes of some diseases, and as the curative remedies in other diseases. The same rule that pertained to dead pertained to living organic matter. Hence long exposure to ammoniacal vapour, by arresting oxidation, produced extreme anæmia and a low depraved condition of the system altogether, with reduced respiration, reduced appetite, reduced muscular power, and reduced energy. On the other hand, in cases where a rapid oxidation of the body was being determined, attended with increase of heat and rapid disintegration of tissue, the administration of ammonia, by arresting these changes, became, in judicious hands, the most powerful and effective of remedies. It checked decomposition by its action on oxygen; it held the blood-fluid by its solvent power as an alkali; and, being volatile, it inflicted no immediate injuries on the structures of the body.

*Two New Preparations from Chloroform—Chloric Ether
and Chloroform Julep.*

By Dr. THOMAS SKINNER, of Liverpool.

(*British Medical Journal*, May 10, 1863.)

There are two physical properties possessed by chloroform which are not fully considered in any of our text books on materia medica; namely—1. Its solubility in alcohol, and subsequent miscibility in water. 2. Its miscibility, if not its solubility, in water.

Lately, while engaged experimenting in order to ascertain the probable composition of Davenport's Chlorodyne, Dr. Skinner accidentally discovered the following valuable facts:—

1. If chloroform be dissolved in rectified spirit of wine, of specific gravity '838, at 60° Fahr. (L. P.), in the proportion of from one to sixteen minims of chloroform in a fluid ounce, the resulting liquid is entirely and freely miscible with water in all proportions. On adding minim by minim of chloroform to the measure of thirty or thirty-two minims to the ounce of the mixture, the solution ceases to be miscible with water in any proportion; the chloroform spontaneously precipitating in small globules, which ultimately coalesce.

2. If chloroform be added to distilled or any good drinking water,

in the proportion of half a fluid drachm to a pint (twenty fluid ounces), and briskly agitated, the resulting liquid is perfectly clear and bright, and no globules of chloroform are precipitated, nor can any be detected with the microscope by a power equal to 250 diameters. On adding more chloroform *gradatim*, the point of saturation is not arrived at until the proportions are a fluid drachm to a pint of water. Sixty-four minims to the pint render the mixture quite opalescent, and much of the chloroform is precipitated. At the point of saturation, a drop of any essential oil shaken with the mixture will determine the separation of the chloroform. If the whole fluid drachm of chloroform be added *at once* to a pint of water, and shaken, the resulting liquid will not be so clear as when it is added gradually.

Bearing these interesting facts in mind, Dr. Skinner resolved to put them to some practical use; and he now suggests the propriety of there being two new official preparations of chloroform, the names and formulæ for which shall be as follow:—

Spiritus Formylis Terchloridi (commonly called chloric ether):—

Chloroform, 3v; rectified spirit of wine, sp. gr. .838 (L. P.),
Oj. Mix. Dose, 3ss to 3ij.

Mistura Formylis Terchloridi (or chloroform julep):—Chloroform, 3ss; pure water, Oj. Mix thoroughly, with brisk agitation, for a minute or two, in a vessel capable of containing double the quantity. Dose, 3ss to 3ij.

A chemical nomenclature has been chosen, in deference to the possible and not improbable fears of the patient.

"The first preparation," says Dr. Skinner, "requires little to be said about it beyond this: that, although it is already very well known to medical men, and to both wholesale and retail druggists, yet, strange to say, its actual composition and the method of preparing it have been kept a secret from the profession, and from the members of the drug-trade as a body. I have been informed that Professor Neligan gives a formula for *chloric ether*, and that there is one wholesale house in London that makes it according to his formula. I can only say that the house alluded to must sell chloroform for chloric ether, as Dr. Neligan's formula is for chloroform, which at one time passed under the cognomen of chloric ether—a less questionable title than that given to the solution of chloroform in spirit of the present day. (See Neligan on *Medicines*, 2nd ed., 1847, p. 257; and subsequent editions—1851, for instance, p. 301.)

"In this town alone there are not two preparations of the so-called chloric ether alike, either in composition or in the invaluable property of being miscible in water. In the April number of the *Pharmaceutical Journal*, Mr. Norman Tate, of this town, has shown that, out of thirteen specimens of chloric ether obtained by him from various druggists in Liverpool and Birkenhead, there is a difference of 9½ per cent. between the strongest and the weakest samples. As regards the formula for chloric ether above given, I can aver, from considerable clinical experience, that if it be prepared from *bond fide* materials, it will equal in medicinal virtue the best chloric ether in the market.

"With reference to the other preparation, the chloroform julep, I believe it has hitherto been quite unknown; but the simplicity of its composition, the pleasantness of its taste, the fragrance of its odour, its great and undeniable power as a diffusible stimulant, and its capabilities as a safe and excellent vehicle for administering other remedies, alkaline, acid, or neutral, bitter, sour, or foetid, need only be alluded to in order to insure it a ready acceptance by the practitioner of medicine. This preparation is reported by some who have tried it to be reckoned a specific for toothache and other painful affections of the mouth and gums. It is simply used as a lotion, wash, or gargle.

"The strengths of the foregoing preparations have been made considerably below their respective points of saturation, partly in order to render the dose less irritating to the mouth and throat, and partly to prevent the separation of the chloroform on the addition of an essential oil or such like medicine to a mixture, particularly if prepared from the julep.

"In conclusion, whether the above formulæ ever become officinal or not, it is to be hoped that a fixed standard will be given to the spirit of chloroform and chloroform julep in the first or second edition of the coming *National Pharmacopœia*."

IV.

REPORT ON PHYSIOLOGY.

Natural Death a Natural Sleep.

By Dr. J. W. BEAUMONT.

(Medical Circular, November 5, 1862.)

In consequence of the injurious effects of the occupations of civilized life, and the diseases engendered by them, few persons can be said to die a really natural death; the "end of man's days" being precipitated by the existence of morbid alterations in some or other of the organs essential to life. Still there *must be* a natural form of death, and it is an interesting question to ascertain in what manner it takes place.

Syncope, asthenia, asphyxia (or apnœa), coma, paralysis, and necrœmia (or death beginning in the blood) are the several modes in which death is said to occur (see Williams's *Principles of Medicine*.) These, however, are all the results of disease properly so called; but there is yet another form or species of death, to which I wish to call attention, occurring exclusively in persons of advanced age—namely, death by *sleep*. This differs from coma in many respects, although it may become blended or lost in it immediately prior to death. Coma is an intense stupor, often accompanied with paralysis or convulsions or other distinct phenomena of disease. But the *death-sleep*, if it may so be termed, resembles ordinary sleep, and at the outset seems to differ in no way from it. The patient at first is simply drowsy, and becomes indifferent to food; the drowsiness or somnolency increases; there is no pain; the system becomes exhausted for want of its accustomed aliment, and ultimately the patient passes gently and quietly into eternity; and so easily is the transition in some cases effected, that it is difficult to say whether he has died, pathologically speaking, from asthenia, syncope, or coma. This is a mode of death of which Dr. Beaumont has during the last ten years met with very many interesting and characteristic examples, in which, indeed, the patient has literally "fallen asleep." The duration of this sleep is usually several days, but it may be prolonged according to the patient's constitutional powers. Dr. Beaumont has in some cases known it extend over three weeks, and it has been a matter of amazement to observers that life should be sustained so long without food. In one case the patient was an old man, who had always enjoyed good bodily health, and whose mental

faculties were good to the last. He manifested no signs of pain, nor anything which could distinctly indicate active disease either in the head, thorax, or abdomen. The chief answer to my inquiries was, that "he slept all his time." He could be roused in the earlier stage, and would reply to questions, but it was much in the same way as would a person who was becoming fatally benumbed with cold; he would immediately after relapse into a dose. In this state he continued a few days, when his sleep became heavy and comatose, and terminated in the sensation of the pulse and of all the functions pertaining to life. Such is the course of a well-marked case. Sometimes, however, there are febrile symptoms, thirst, dryness of the mouth, accelerated pulse, &c. In a case of this kind which occurred to me recently, the patient being about eighty-four, the sleep was at first the only noticeable peculiarity, and no febrile symptoms appeared for at least a week. It was a prolonged case, and for one or two weeks previously to death, these symptoms had subsided altogether, and that event took place characteristically, the patient seeming to die in an ordinary slumber. Many deaths commonly attributed to "old age," or to a "decay of nature" (senilitas) take place in this way.

In the volume of unerring truth, it is recorded that David was "old and stricken in years," and that he "slept with his fathers," and was buried in the city of David. Solomon, too, in due time, "slept with his fathers," and the decease of Hezekiah and others is recorded in similar words. This phraseology is elegant and poetical, but it is something more; and although many of those who are said to have "fallen asleep" did not do so always from advanced age, yet the phrase is at least an *allusion* to the natural mode of human death.

The conclusion is, then, that when the bodily organization has attained its maximum of vital capability, and when further nutrition is impossible, a sleep ensues, soft and gentle at first, and not to be confounded with the coma of disease; and that it increases in intensity until at last the spirit is quietly disimprisoned from the body. This Dr. Beaumont regards as purely natural death, or death occurring as a natural *sleep*.

Report on Suspended Animation.

By a Committee of the Royal Medico-Chir. Society.

(*Proceedings of the Royal Medico-Chirurgical Society, July 12, 1862.*)

This committee consisted of Drs. Williams, Kirkes, Harley, Sanderson, Brown-Séquard, Hyde Salter, Sieveking, and Mr. Savory.

This inquiry was conducted—

By means of experiments upon living animals;

By means of experiments upon the dead human body.

In investigating anew the subject of apnoea by means of experiments on the lower animals, it seemed expedient to observe, in the first place, the principal phenomena of apnoea in its least compli-

cated form—namely, when produced by simply depriving the animal of air.

The principal facts to which attention was directed during the progress of the apnoea thus induced were—

The duration of the respiratory movements ;

The duration of the heart's action.

The duration of the heart's action was observed—

(a) In relation to the duration of the respiratory movements.

(b) In relation to the time after the stoppage of the breathing.

From the experiments performed it appeared that in the dog the average duration of the respiratory movements after the animal has been deprived of air is 4 min. 5 sec., the extremes being 3 min. 30 sec. and 4 min. 40 sec. The average duration of the heart's action is 7 min. 11 sec., the extremes being 6 min. 40 sec. and 7 min. 45 sec.

From these experiments it appears that on an average the heart's action continues for 3 min. 15 sec. after the animal has ceased to make respiratory efforts, the extremes being 2 min. and 4 min. respectively.

Rabbits on an average ceased to make respiratory efforts in 3 min. 25 sec. Their heart's action stopped in 7 min. 10 sec. ; consequently the interval between the last respiratory effort and the cessation of the heart's action was 3 min. 45 sec.

The next question investigated was—the period after the simple deprivation of air at which recovery is possible, under natural circumstances, without the aid of any artificial means of resuscitation.

The experiments performed led to the conclusion that a dog may be deprived of air during 3 min. 50 sec., and afterwards recover without the application of artificial means ; that a dog is not likely to recover, if left to itself, after having been deprived of air during 4 min. 10 sec.

The force of the inspiratory efforts during apnoea was observed in the experiments to be so great that it was determined to measure them. They were found to be capable, in the dog, of raising a column of mercury four inches. It appeared, moreover, that their force increases up to a certain period.

In other experiments, plaster of Paris, and even mercury, were thus drawn upwards into the minute bronchial tubes.

It is easy to understand, therefore, how foreign bodies may be drawn into the lungs in cases of drowning, and the importance of this fact in the consideration of the pathology and treatment of apnoea.

The committee next passed on to the subject of drowning.

The first question investigated was—For what period can an animal be submerged, and yet recover without the aid of artificial means ?

It was found as the result of numerous experiments on dogs, that in striking contrast to the previous ones, $1\frac{1}{2}$ minute's immersion in water suffices to destroy life.

Other experiments satisfactorily showed that the difference of

time between simple apnoea and that by drowning is not due to submersion, or to depression of temperature, or to struggling, but that it is connected with the fact, that in the one case a free passage of air out of the lungs, and of water into them, is permitted; in the other, the exit of air and the entrance of water are prevented.

There can be no doubt, from other considerations put forward, that although both these circumstances are concerned in producing the difference observed, yet that it is mainly due to the entrance of water and the effects thereby produced.

The treatment of apnoea was next considered.

For conclusions respecting artificial respiration, the Committee refer to the second portion of the Report.

Many other methods of resuscitation which have been recommended were employed, including actual cautery, venesection, cold splash, alternate application of hot and cold water, galvanism, puncture of the diaphragm.

Although some of the above means were occasionally of manifest advantage, no one was of such unequivocal efficacy in a sufficient number of cases as to warrant the Committee in specially recommending its adoption.

The experiments upon the dead subject were made with a view to determine the value of the various methods which have been employed for alternately compressing and expanding the cavity of the chest in such a manner as to imitate the natural movements of the thoracic walls in breathing. The following methods have been investigated:—

1. Pressure exerted by the hands on the anterior wall of the thorax, the body being in the prone posture. Such pressure has for its object to expel a portion of the air contained in the chest: on relaxing the pressure, the chest expands and air enters.

2. The postural or so-called "ready" method, described by Dr. Marshall Hall, which consists essentially in "turning the body gently on the side and a little beyond, and then briskly on the face alternately;" and in making pressure along the back of the chest each time the body is brought into the prone position.

3. The method of Dr. Silvester, in which the action of the pectoral and other muscles passing from the shoulders to the parietes of the chest in deep inspiration is imitated. An inspiratory effort is produced by extending the arms upwards by the sides of the head; on restoring them to their original position by the side of the body, the expanded walls are allowed to resume their previous state, and expiration takes place, the quantity of air expelled being in proportion to that which had been previously inspired.

It being necessary to measure the flow of air in and out of the respiratory cavity under conditions of pressure closely resembling those which exist in natural respiration, no means of measurement could be used, which, in its working, would offer any appreciable resistance to the passage of air. With this consideration in view, an instrument designed by Dr. Sanderson was employed. (The instrument was exhibited to the Society.)

General results.

1. As regards the volume of air which can be expelled from the thorax by compression of its walls, and inspired by the elastic expansion consequent on relaxation of the pressure, it was found—

(a) That pressure by both hands on the lower third of the sternum in the adult male subject usually displaced from 8 to 10 inches of air.

The pressure actually exerted amounted to about 30 lbs. It was, therefore, not greater than might be safely applied to the living subject. The volume of air expelled varied from 8 cubic inches to 15 cubic inches.

(b) That pressure made in the same manner on the upper part of the sternum usually displaced 2 or 3 cubic inches less than pressure on the lower part.

(c) That pressure exerted by one hand on the upper part, by the other on the lower part of the sternum, produced about the same results as were observed in *a*.

In this case the whole amount of pressure did not exceed that exerted in *a*.

(d) That the pressure of a weight laid on the lower third of the sternum produced similar results according to its amount.

(e) That lateral pressure exerted on the ribs or costal cartilages of both sides simultaneously was in no instance more effectual.

(f) That compression by a broad bandage encircling the chest, the ends of which were crossed over the sternum, and drawn in opposite directions by two persons, produced no greater effect than pressure with the hands on the sternum or sides.

2. As regards the whole amount of exchange of air produced by the method of Dr. Marshall Hall, "to imitate respiration," it varied much, according as the subject was favourable or the contrary; sometimes not exceeding a few cubic inches, but never exceeding 15 cubic inches.

3. As regards Dr. Silvester's method, it was found that on extending the arms upwards, a volume of air was inspired into the chest, which varied, in different subjects, from 9 to 44 cubic inches, and it was observed that the results obtained in successful experiments on the same body were remarkably uniform, in which respect, as well as in their amount, they contrasted with those obtained by the method of Dr. M. Hall. On restoring the arms to the side, the quantity of air expelled was generally nearly equal to that previously inspired, occasionally less.

In the treatment of apnoea generally, the Committee offer the following suggestions:—

That all obstruction to the passage of air to and from the lungs be at once, so far as is practicable, removed;—that the mouth and nostrils, for example, be cleansed from all foreign matters or adhering mucus.

That in the absence of natural respiration, artificial respiration by Dr. Silvester's plan be forthwith employed in the following manner:—The body being laid on its back (either on a flat surface, or, better, on a plane inclined a little from the feet upwards), a firm

cushion or some similar support should be placed under the shoulders, the head being kept on a line with the trunk. The tongue should be drawn forward so as to project a little from the side of the mouth. Then the arms should be drawn upwards until they nearly meet above the head (the operator grasping them just above the elbows), and then at once lowered and replaced at the side. This should be immediately followed by moderate pressure with both hands upon the lower part of the sternum. This process is to be repeated twelve or fourteen times in the minute.

That if no natural respiratory efforts supervene, a dash of hot water (120° Fahr.) or cold water be employed, for the purpose of exciting respiratory efforts.

That the temperature of the body be maintained by friction, warm blankets, the warm bath, &c.

In the case of drowning, in addition to the foregoing suggestions, the following plan may be in the first instance practised:—Place the body with the face downwards, and hanging a little over the edge of a table, shutter, or board, raised to an angle of about thirty degrees, so that the head may be lower than the feet. Open the mouth and draw the tongue forward. Keep the body in this posture for a few seconds, or a little longer if fluid escapes. The escape of fluid may be assisted by pressing once or twice upon the back.

A Parallel between the Cerebro-Spinal and Sympathetic Nervous Systems.

By Professor CLAUDE BERNARD.

(*Medical Times and Gazette*, Aug. 17, 1861.)

In a lecture (one of a series) on the spinal cord, delivered at the College de France in the summer session of 1860, Professor Bernard, speaking upon this topic, says:—

“The functions of the sympathetic nerve, and of the cerebro-spinal system, have long been viewed as entirely different in their nature, organic life being the sphere of activity of the first, and animal life of the second. But the recent progress of physiology has shown so absolute a distinction to be unfounded. There exist certain expressions, still in use among medical men, which ought to be expunged from the vocabulary of science, and the words ‘sympathetic nerve’ or ‘cerebro-spinal system’ evidently belong to this class. The difference exhibited by the two systems in their outward appearance and anatomical disposition has been the principal source of this error. Struck with the presence of ganglia on the organic nerves, anatomists were led to consider them as an apparatus entirely distinct from the voluntary nerves; and Bichat himself, adopting in this respect the opinions in force among his contemporaries, believed each ganglion to be the centre of an independent nervous system.

“The notions which formerly prevailed on this subject are now

completely laid aside, and it has been proved beyond the possibility of a doubt, that, although the ganglia enjoy certain special powers, they must not be viewed as independent sources of nervous action. The sympathetic nerve is known to arise from the spinal cord; modern micrographers believe it to be derived from a distinct order of cells; and physiology fully confirms these anatomical data, for the cervical branch of this nerve is completely paralyzed by the division of the anterior roots of the seventh and eighth spinal pairs. We shall, no doubt, soon be enabled to prove, by similar experiments, that all the branches of the sympathetic nerve are derived from various regions of the spinal cord; the actual tendencies of physiology point to this result.

"But the origin of the sympathetic nerve was not the only argument adduced in support of its existence as an independent system; its action on the muscular fibres was also believed to be different from that of the voluntary nerves, the contractions excited by the latter being instantaneous, while those excited by the former are slow in making their appearance. Either, therefore, the nerve-fibres conduct differently in the sympathetic system, which is not probable, or the histological elements placed under its control do not obey at once the impressions which reach them; we have already stated the latter opinion to be our own.

"The nervous system, therefore, contains only two great divisions—sensitive and motor nerves: it matters little whether the subject is conscious of their action or not, their properties in both cases being identically the same. The sensitive apparatus is endowed with consciousness in the cerebro-spinal system, and does not enjoy this property in the organic nerves; but the difference is not an absolute and unconditional one; for the viscera frequently acquire a high degree of sensibility in acute disease, and, on the other hand, the cutaneous surface, in paralytical subjects, is totally insensible to external impressions; that is to say, the feeling of pain no longer exists, but the reflex actions which are still produced inform us that the sensitive nerves still retain a part at least of their usual properties.

"With respect to the motor nerves, our experimental researches have led us to distinguish two kinds of these; the first being destined to reduce the capacity of the vessels, the second to increase it. We know the former to be derived from the sympathetic nerve, and believe the latter to spring from the cerebro-spinal axis. To those of the first class we give the name of vasomotor nerves; we know them, in fact, to produce contraction in the vessels by acting upon muscular elements; but the powers of the antagonist system are infinitely more difficult of comprehension; it appears to stimulate the dormant activity of the tissues to which it spreads, creating secretion in glands, contraction in muscles, and phenomena of a different nature in other parts. Whether the nerves which give rise to these effects and those which dilate the vessels are identically the same, is a matter of doubt. You are aware that contraction in muscles and secretion in glands usually coincide with a turgid state of the capillaries; but these phenomena are not inseparably con-

nected; the circulation of a muscle may be accelerated, without giving rise to contractions, and contractions may take place without exerting the slightest influence on the local circulation. The same being the case in glands, it may be questioned whether these various effects are all produced by the same class of nerves.

"There exists a circumstance in this respect, which we have hitherto been at loss to explain: we allude to the influence which both the sympathetic nerve and the tympanic cord appear to exert upon the salivary glands. When the motor nerve is galvanised the circulation is accelerated, and secretion takes place; but when the sympathetic nerve is excited, the vessels contract, the passage of blood in the vessels is retarded, and yet the power of secretion continues. The properties of the fluid produced are, however, not the same.

"The results of this experiment appear to justify the conclusion that two distinct orders of nerves preside over the functions of glands; but we ought not, perhaps, to place an implicit reliance on the stimulus employed, viz., galvanism; for M. Dubois Reymond has proved that on electrifying the nerves an electro-tonic state is produced, which does not act upon the muscles, but upon the nerves which lie in the vicinity; they are liable, in fact, to receive a part of the superabundant electricity condensed in the neighbouring trunk. Let it be supposed, for instance, that a current passes through a nerve which lies in contact with another divided nerve, contractions will appear in the muscle connected with this latter branch, although no direct impulse has been conveyed to it; the proximity of another nerve, strongly impregnated with the electric fluid, being sufficient to create a participation in its effects.

"It seems, therefore, preferable to employ other kinds of stimuli in experiments of this nature; common salt, for instance, is a powerful excitant of the nervous system, and is perfectly free from any similar inconvenience; there is no difficulty in proving this at once.

"It would, therefore, perhaps be necessary to modify, up to a certain point, the opinions which we had formerly adopted on this subject. There appears to exist only one nerve which acts upon the secreting powers of the gland, and that nerve is derived from the cerebro-spinal system; the effects produced by the galvanization of the sympathetic nerve being probably due to the electro-tonic state which follows. In fact, when a powerful galvanic current is brought to bear upon the nerve which dilates the vessels, the antagonist system is sometimes also excited, if the current is sufficiently strong. If, for instance, we gently stimulate the tympanic cord, the circulation is at first accelerated, and the secretion commences; but if the intensity of the current is gradually increased, the vessels contract, the passage of the blood is retarded, yet the secretion still continues to flow. In this experiment the sympathetic nerve has evidently been excited by the electro-tonic state of the tympanic cord; but on reversing the experiment—on galvanizing with great intensity the sympathetic branch which extends to the sub-maxillary gland, a similar influence is exerted on the motor nerve; circulation is at

first impeded, and the blood contained in the veins grows black ; but on continuing the experiment, the secreting elements of the gland will at last be excited, and the saliva will begin to flow. We, therefore, consider the action of salt upon the nerves to be a closer approach to the physiological state ; and, in our opinion, all the experiments hitherto made on secretion require a complete revision ; galvanism having almost invariably been employed in these researches, the results obtained up to the present time can no longer be relied upon, and the opinions expressed in a former lecture will, perhaps, be contradicted by future experiments.

"The preceding considerations are equally applicable to the muscular system. When the nerve which presides over the contractions of a muscle is excited, the vasomotor branches simultaneously obey the impulse ; the muscular fibres and the vessels contract at the same instant, and the venous blood assumes a dark hue. It would, therefore, be desirable to separate these two classes of nerves, in order to observe the effects produced by each of them in its individual capacity ; but in the greater part of the body to do this is impossible. The voluntary and involuntary nerves are united in a single trunk, and cannot be separately excited. The muscles of the face alone are favourably disposed for this operation, being supplied with nerves from the cranial pairs, which do not mingle with each other immediately after their origin. The masseter is one of the muscles which offer the best opportunity for making this experiment on account of the peculiar anatomical distribution of its nerves. The ramifications of the sympathetic system which extend to this muscle being divided, the venous blood grows scarlet ; but on galvanizing the peripheral extremity of the cut nerve the blood regains at once its primitive appearance, the muscular fibres remaining, however, in a state of perfect tranquillity. From this experiment it may be inferred that the chemical composition of the blood, which is evidently connected with the process of nutrition in the tissues, is not modified in all cases by this vital condition. We know, in fact, that muscles entirely separated from the body, and totally deprived of circulation, still retain the power of entering into contraction when excited ; the physiological act in this instance is altogether independent of the state of the blood.

"The system of organic life appears, therefore, to confine its action to the vessels. But the reciprocal influence of sensitive and motor nerves in the cerebro-spinal apparatus is too well known to require a proof, nor is it to be supposed that reflex actions are confined to the voluntary nerves. The vasomotor apparatus equally enjoys this important property. The influence of cold on vascular contraction evidently belongs to this class of phenomena ; and you are aware that on dividing on one side the cervico-cephalic branch in animals subjected to the process of congelation the effects of cold are immediately felt on the non-mutilated side, and are slow in making their appearance on the other.

"It is highly probable, gentlemen, that in the difficult and complicated study of the nervous system we meet with two distinct orders of nerves. The vessels are placed under the influence of the

first, while the histological elements obey the power of the second; nutrition depends on the former, and physiological activity is aroused by the latter. The sympathetic nerve may therefore be viewed as a complementary apparatus placed by the side of the cerebro-spinal system. Both are derived from the same origin, both enjoy similar properties, but the elements on which their action is exerted are of a different nature."

On the Functions of the Cerebellum.

By Dr. R. WAGNER and Dr. E. BROWN-SÉQUARD.

(*Journal de Physiologie*, 1861.)

Dr. Wagner adopts a double method of inquiry. He appeals (1) to cases in which the cerebellum has been injured by disease or accident; and (2) to vivisections practised on the lower animals. Dr. E. Brown-Séguard's remarks occur as a criticism upon Dr. Wagner's paper.

Dr. Wagner thus sums up his conclusions as to the functions of the cerebellum :—

1. The cerebellum has nothing whatever to do with the transmission of sensitive impressions.
2. Although in most cases of disease of the cerebellum morbid sensations are present, these are due to compression of nerves at their origin, or of the fibres of the spinal cord. The vomiting, which is so frequent a symptom, is explained in the same way.
3. The cerebellum does not preside over reflex action, nor does it receive the terminations of centripetal nerves.
4. The cerebellum is not a central organ either for general sensibility or for the special senses. When the latter are affected, they are so secondarily.
5. The cerebellum has no part in the psychical functions.
6. The cerebellum is an organ exclusively motor, for the muscular apparatus of animal, and probably, too, of organic life.
7. Amongst motory functions, it is certain that the cerebellum has an essential part in the co-ordination of symmetrical movements of the body, and notably of the movements of progression.
8. Vivisections and pathological cases show that the cerebellum may become the point of departure of a direct excitation of certain muscular organs, notably of the abdominal viscera, specially of the genital organs, and probably also of the heart.

In some remarks on the minute structure of the cerebellum, Wagner seems to believe that a certain number of cell-processes lose themselves in the finely-granular stroma, which by some is considered as a connective tissue (Virchow and Kölliker), by him (and previously by Henle) as a diffused ganglionic substance not condensed into cells.

Dr. Brown-Séguard's conclusions are these :—

1. The cerebellum is neither a centre of perception of sensitive impressions, nor a place of passage for the conductors of these

impressions. But in consequence, perhaps, of pressure on the pons, and perhaps by influencing other parts of the brain, anæsthesia is sometimes a symptom of a lesion of the cerebellum.

2. Vomiting is more frequently a symptom of lesion of the cerebellum than of the pons Varolii, and in many such cases cannot be accounted for by pressure on the pons. It is produced by irritation of the cerebellar tissue.

3. Convulsions may be produced by inflammation of the tissue of the cerebellum. Cases prove this.

4. Although this organ cannot be viewed as a centre of vision or hearing, it has a special influence on vision. In sixty cases collected by Brown-Séquard, in which the cerebellum was the seat of a lesion, there was amaurosis in one or both eyes. In many of these the latter could not be accounted for by pressure on the tubercula quadrigemina. Like vomiting, amaurosis may be caused by irritation of the cerebellum.

5. Brown-Séquard agrees with Wagner's opinion as to the psychical relations of this organ.

6. The cerebellum is *not* an organ exclusively motor for the muscular apparatus of animal life.

7. It is certain that the cerebellum has *not* an essential part in the co-ordination of the symmetrical movements of the body. The experiments even of Wagner himself prove that the loss of co-ordination may be restored in a few days or weeks. Slight pricks produce as much disturbance of the voluntary movements as ablation of the organ. The disturbance is due to irritation of the various peduncles of the medulla oblongata and the pons. Vivisections prove, 1st, That notwithstanding absence of the cerebellum, voluntary movements can be regularly performed; 2nd, That without lesion of the cerebellum, irritation of the neighbouring parts can produce disorders of movement even more considerable or more complicated than those which we see after removal of the cerebellum. Pathological facts support these conclusions.

8. In reference to an influence over abdominal viscera, everything in this field of experiment needs revision. *The cerebellum possesses none of the functions which have been attributed to it.*

A New Motor System of the Heart.

By Professor VON BEZOLD, of Jena.

(*Berlin Med. Centralzeitung*; and *British Medical Journal*, Feb. 7, 1863.)

Professor von Bezold of Jena states that he has, in the course of some researches on the motion of the heart, discovered a new source of motor nerves of this organ, which connect it much more intimately and importantly than the cervical sympathetic or even the par vagum with the cerebro-spinal system. The following results were arrived at by him from experiments on animals paralysed by woorara.

1. When the pneumogastric and sympathetic nerves were divided

in the neck, so as to cut off the hitherto recognised condition of nervous influence from the central organs to the heart, irritation of the medulla oblongata produced a very marked increase in frequency of the pulse and an uncommon augmentation of the pressure of the blood in the arteries—the latter being sometimes more than doubled.

2. When, further, the spinal cord was divided in these animals at any point above the seventh cervical vertebra, the activity of the heart was almost immediately diminished: the pressure of the blood in the carotid was diminished to three-fourths, the beat of the heart became weak, and its sounds almost inaudible, &c. On the other hand, when the division was made in the neighbourhood of the third or fourth dorsal vertebra, no such effect was produced. Hence the centre of innervation does not extend below the fourth dorsal vertebra.

3. If, after dividing the cord above the seventh cervical vertebra, the medulla oblongata or the portion of the cord above the division be irritated, however violently, whether mechanically or by electricity, no change is produced in the pressure of the blood or in the frequency of the pulse. But if the portion of the cord on the distal side of the division be irritated, the blood-pressure and the heart's action, which have been diminished by the section, may be raised to or even above the normal height. Hence the motor nerves (exclusive of the par vagum and cervical sympathetic) do not come off above the seventh cervical vertebra.

4. If, in partially poisoned animals, the medulla oblongata and the peripheric portion of the divided vagi be simultaneously irritated, the results are, not an increase, but a diminution, of the pressure of the blood; not an acceleration of the pulse, but a retardation or even cessation. Bezold derives the following conclusions from his experiments.

a. He believes he has ascertained the existence of a new central organ for the motions of the heart, having its seat neither in the medulla oblongata nor in the brain.

b. Its fibres run through the cervical spinal cord, and pass out between the seventh cervical and fifth dorsal vertebrae.

c. They very probably pass through the lower cervical and upper dorsal sympathetic ganglia, and enter the breast as the lower and middle cardiac nerves.

d. This central motor organ normally furnishes three-fourths of the entire propulsive force of the heart; when it is abnormally irritated, the energy of the contractions of the heart may be increased sixfold.

e. This cardiac nervous system, apparently having its seat in the medulla oblongata, stands in a reflex connexion with the sensory cerebro-spinal fibres.

f. Various poisons, especially digitalis and strychnia, increase and strengthen the heart's beat, after division of the vagus, by increasing the irritability and activity of this cerebro-spinal cardiac nervous system.

On the Calorific and Vascular Nerves of the Grand Sympathetic.

By Professor CLAUDE BERNARD.

(*Gazette Hebdomadaire de Médecine et de Chirurgie*, Août 15 and 29, 1862.)

In a series of papers recently presented to the Paris Academy of Sciences, Professor Claude Bernard shows very conclusively that the sacro-motor nerves of the extremities are as distinct from the common motor nerves as he has shown them to be in the head and neck.

Having opened the spinal canal in dogs, he divided as they left the cord all the origins of the sacro-lumbar plexus (sometimes on one side and sometimes on the other) which supplies sensation and motion to the hinder extremity. The limb became completely paralysed, but no calorification or vascularization was observed, the temperature on this side often, indeed, diminishing. When only the posterior and anterior roots were divided, corresponding abolition of sensation or of motion occurred; but in neither case was there any vascularization or change of temperature in the limb. In a dog in which complete paralysis of the left hind leg was produced by division of the origins of the sacro-lumbar plexus, the sciatic nerve was afterwards divided. Its origin having been already divided, this subsequent section was not felt, and added nothing to the paralysis of motion and sensation that already existed; but vascular and calorific phenomena immediately followed, the temperature of the limb steadily rising until it was from 6° to 8° C. higher than that of the opposite one, and so continued until the death of the animal next day.

The experiment was repeated a great many times with exactly the same result. It is evident, therefore, that nerves influencing these functions must have become adjoined to the motor and sensitive nerves in the short interval between their issue from the canal and the point where the sciatic was divided. It is only the sympathetic, placed on the sides of the spinal column, which could thus become joined to these nerves; and M. Bernard, in another experiment, destroyed the ganglion of the sympathetic and its filaments, which lay upon the side of the fifth and sixth lumbar vertebræ, leaving the nerves of the sacro-lumbar plexus entirely intact. An excess of temperature in the limb was immediately observed, and during the three days the animal lived, the paw of the side operated upon was from 5° to 8° hotter than the other, no paralysis whatever being present. The conclusion to be drawn is, that there are three distinct descriptions of nervous influence.—1. The sensitive, due to the posterior roots of the sacro-lumbar plexus; 2. The motor or muscular, belonging to the anterior roots; and 3. The vascular and calorific, due to the sympathetic.

Experiments made in the anterior extremities yielded exactly the same results.

On the Physiology of the Sub-Maxillary Ganglion.

By Professor CLAUDE BERNARD.

(Medical Times and Gazette, Sept. 19, 1862.)

All movements regulated by the sympathetic nerve are reflex movements, and as such quite independent of volition. It is generally admitted that the brain and the spinal cord are the exclusive centres of all reflex movements, and that the ganglions of the sympathetic nerve are not to be considered as centres for the production of reflex actions, notwithstanding the presence of nerve-cells in their texture. M. Bernard has, however, proved that the submaxillary ganglion is the seat of reflex actions, which have nothing to do with the cerebro-spinal system. In man and all animals provided with a salivary apparatus, there exists in the course of the lingual nerve a small ganglion which has anatomical and physiological relations with the nervous system of the submaxillary gland. This ganglion varies as to volume and situation in the different animals, but it always constitutes a sort of ganglionic or sympathetic bridge, which, going from the lingual nerve to the chorda tympani, can physiologically connect the tongue, or rather the bucco-lingual mucous membrane, with the submaxillary gland. By means of this bridge formed by the submaxillary ganglion and the fibres emanating from it, reflex actions of the tongue upon the submaxillary gland may be produced without intervention of the brain. The tongue is connected with the submaxillary gland by two sorts of concentric nervous arches; the one, and the larger (viz., the lingual nerve and the chorda tympani), passing through the brain; and the other, and shorter one, passing through the submaxillary ganglion. Now, there appear to be two sorts of reflex influences destined to act upon the submaxillary gland, which correspond with the two nervous courses. The first, which passes through the brain is conscious, and put in action especially by the gustatory function of the tongue; the second, which is unconscious, is transmitted by the submaxillary ganglion, and seems to be particularly excited by the conditions of dryness or humidity of the bucco-lingual mucous membrane. But the submaxillary ganglion has not only the power of propagating reflex actions, which may by its means arrive at the submaxillary gland, without passing through the brain; it also seems to possess a special influence upon the intermittence of the salivary secretion; for after the section of this ganglion the secretion of the gland becomes continuous, although it may still increase in intensity if excitants of taste are applied to the tongue. The submaxillary ganglion loses this property of being a centre of reflex actions for the submaxillary gland some time after it has been separated from the brain; after which the gland, being thus entirely deprived of nervous influence, secretes permanently, instead of entering into a state of functional repose. This fact, which merits the special attention of physiologists, may go far to show that all our ideas concerning the way in which the nerves act upon the organs are

erroneous. The nerves would no longer seem to be the excitors of action, but would rather seem to refrain it; and the organs, the functional power of which would be idio-organic, would appear to be able to manifest their action only if the nervous influence relaxed for a moment its regulating force.

On Active Dilatation of the Vessels.

By Professor CLAUDE BERNARD.

(*Medical Times and Gazette*, June 22, 1861.)

In the lectures on the spinal cord, to which reference was made a few pages back, M. Claude Bernard made the following remarks upon this subject:—

“The action of the sympathetic nerve upon the vessels, as you are aware, is rendered apparent by variations of considerable extent in their capacity. It causes them to contract when galvanized, and allows them to dilate when divided. In short, on comparing the effects of this latter operation with those produced on the voluntary muscles by the division of the corresponding motor nerve, we are at first sight led to suppose this enlargement of the vessels to be occasioned by the sudden paralysis of their contractile elements. What are, in fact, the visible results of the experiment? A nerve which arises from the anterior tracts of the spinal cord, and spreads to the most remote ramifications of the arterial system, appears to exert a remarkable influence upon the temperature of the body. If one of its leading branches is divided, circulation is accelerated in the corresponding parts, the secretions flow more abundantly, and the temperature rises. If, on the contrary, it is galvanized, the reverse immediately takes place. Now this nerve evidently belongs to the motor system, for its peripheric, and not its central extremity retains its usual properties when it has been divided. It seems, therefore, natural enough to conclude that the elevation of temperature and other effects which follow this operation arise from an afflux of blood to the parts thus withdrawn from its influence, and that this exaggeration of the vascular phenomenon is due to the paralysis of the capillary vessels; in other words, to their passive distension. This explanation, which has been adopted by several authors, appears to us insufficient, not to say unsatisfactory. We have ascertained, in fact, that after the experiment the blood which passes into the venous system is warmer than that which flows through the arteries. There exists, therefore, in these phenomena something beyond a mere passive distension of the vessels. We meet here with an action of a different and hitherto unknown kind.

“In our experimental researches on secretion, we were led to the discovery of a power which acts in opposition to the influence of the sympathetic nerve, and is set at liberty by the division of this trunk. We shall no doubt make ourselves easily understood by referring to the alternate contractions and dilatations of the pupil, which first drew the attention of physiologists to this sub-

ject. When the pupil contracts, after the cervico-cephalic branch of the sympathetic nerve has been divided, it obeys the impulse of an antagonist power, which is attributed to the ciliary ramifications of the third pair. In short, the contraction of the iris is not a passive, but an active phenomenon; and the existence of circular fibres, which evidently fulfil this purpose, leaves no doubt on this point. The variations observed in the capacity of vessels appear to belong to the same class of phenomena, although contraction takes place in the first case and dilatation in the second. Nor is this a mere hypothesis, unsupported by positive facts. The active dilatation of the vessels is one of the most indisputable truths which have reached our knowledge. We have proved by direct experiment that there exist two orders of vasomotor nerves acting in opposition to each other, and that their properties are in most cases called into play by reflex actions. Our opinion rests upon the following experimental data.

"When the sympathetic branch which extends to the sub-maxillary gland is divided, an unusual flow of blood to the part immediately takes place. But a similar result is obtained, without injuring the sympathetic nerve, by merely galvanizing the corda tympani; these two nerves are, therefore, opposed to each other, and the dilatation which occurs in the vessels, when the influence of the organic system has been suppressed, is due to the active interference of the tympanic nerve, liberated from its usual antagonist. Besides, when both nerves have been simultaneously divided, a passive distension of the capillaries really takes place, the vessels being thus entirely left to themselves; and the intensity of this phenomenon being accurately measured, we find it to be greatly inferior to the effects of galvanism applied to the tympanic cord, the sympathetic nerve being left untouched. It is hardly necessary to say that on exciting the motor nerve of the gland after destroying its antagonist, the effects produced reach at once the highest limits which they are capable of attaining. The recent discovery of the motor nerve of the parotid gland renders the demonstration complete; and the existence of two distinct classes of vasomotor nerves is a fact which now lies beyond the possibility of a doubt. You must excuse us, gentlemen, for dwelling at considerable length upon this point; for, according to all received notions (up to the present time), contraction is the active or tonic state of all tissues; and to consider the opposite condition as anything but a passive effect, will probably appear a paradox, at first, to most physiologists; nor are we prepared at present to explain the manner in which this active enlargement of the vessels is effected.

"We turn now to another part of the question. The two different kinds of vasomotor nerves may be supposed to arise from the sympathetic nerve, or it may be presumed, on the contrary, that they are derived from distinct sources. Anatomy seems to support this latter view; the constrictor nerves invariably belong to the organic system, while their antagonists, in every known case, arise from the cerebro-spinal axis; the nervous filament which presides over the functions of a gland is always a ramification of some

motor branch; the corda tympani is derived from the facial, and the parotidian nerve evidently belongs to the same trunk, although apparently connected with the fifth pair; for the division of the facial, in its upper part, entirely deprives this latter nerve of its characteristic properties. It would be useless to endeavour, in the actual state of our knowledge, to solve the problem; we shall perhaps, on a future occasion, meet with a more favourable opportunity of discussing it. We must, however, draw your attention to an important point: we allude to the peculiar slowness with which (according to all observers) the organic system transmits the impressions made by stimuli; when the pupil, for instance, is dilated by the application of galvanism to the sympathetic nerve, the effect only takes place by degrees and it persists some time after the stimulus has ceased to act. The nerves which cause the vessels to contract being evidently derived from this source, you will not be surprised to find that their action is strictly the same; but the nerves which dilate the vascular system—the motor nerves of secretion—are in this respect closely allied to those of organic life: they produce the effects which slowly appear, and slowly depart. Some physiologists, therefore, believe them to be ramifications of the sympathetic nerve, probably derived from some of the numerous anastomoses which unite the two systems of animal and organic life: we do not, however, consider this as fully proved by the preceding remarks; for the motor nerves of glands, acting as they do upon the non-striated muscular fibres, ought not perhaps to be compared in this respect with those which extend to the voluntary muscular element; the impression is perhaps transmitted with equal rapidity on both sides, but is not so readily obeyed in the first case as in the second. In short, we are at a loss to determine whether the properties of the nerve, or those of the peculiar tissue to which it spreads, are to be viewed as the cause which retards the appearance of contraction when exerted."

On the Coagulation of the Blood.

By Professor GULLIVER.

(*Medical Times and Gazette*, Feb. 28, 1862.)

In the lectures from which we have taken the previous article, we find these remarks upon this subject. Professor Gulliver says:—

"The interesting question of the cause of the coagulation of the fibrin still remains so problematical that it would be little profitable to employ our time in a review of the numberless speculations which have been entertained concerning this phenomenon. But in our fondness for anything of Hunter's, we cannot dismiss his opinions without notice; nor can we withhold a due respect to the arbiters of the Astley Cooper prize essay, and Dr. Richardson's valuable work for which that prize was awarded.

"*Doctrines of Mr. Hunter and Dr. Richardson.*—The language of Hunter's great book on the Blood is, perhaps, more vague than

it would have been had he lived to see its publication completed. He says that blood is an inorganized substance, though ready to become organized according to the stimulus of the surrounding parts (iii., 36, 113, 119). In his Surgical Lectures he observes that coagulation of the blood appears to be that process which may be compared with life in the solids, and that this disposition to coagulate, when out of the vessels, or retained in them without motion, is one of the effects of the life of the blood (i., 236). And, in short, that the coagulation arises from the living principle of the blood, for if that be destroyed it does not coagulate at all; coagulation being that process which may be compared to life in the solids (iii., 113). Accordingly, he endeavours to prove (iii., 113) that the coagulation of the blood is analogous to the action of muscles, which we know depends on life, and that certain causes of sudden death will at once kill the blood and body together, and so prevent both the post-mortem contraction of muscles and coagulation of the blood. Hence, as far as we can gather from his writings, his opinion was, that the cause of the coagulation is an act of life. As facts in support of this view, he states, that in animals killed by lightning and by hunting, there is neither coagulation of the blood nor contraction of the muscles after death; and adduces instances in two deer which had been run till they dropped down and died (iii., 14). He mentions other forms of sudden death as having the same effect. To the student of his works—which we all ought to be—the firm conviction which these facts, and his leading opinion connected therewith, so long had on Hunter's mind must be remarkable. They form the warp and woof of his theory, always more or less displayed whenever he discourses on the principles of the blood, and were as distinctly announced in his Surgical Lectures as in his last great book. He appears, also to have had an opinion that adhesion between parts of an animal is a proof of the life of both those parts. Thus, the blood coagulates for the union: the simple act of coagulation, apart from its causes, is an operation of life proceeding upon the same principle as union by the first intention (iii., 34). It is well known how he transplanted teeth and cock's spurs, and saw them become fixed to their new seats, and his inference accordingly. Dr. Richardson, on the contrary, concludes, from many ingenious experiments and observations, that the cause of the coagulation of the blood is simply a chemical one—an escape from the fluid blood of the volatile alkali. Hence, we have the vital hypothesis of Mr. Hunter and his disciples, and the chemical one of Dr. Richardson and others; and it is remarkable that Mr. (now Sir Benjamin) Brodie noticed the coagulation of the blood as a 'chemical change.'

*“Review of the Vital and Chemical Doctrines.—*Before attempting a judgment of the value of these two very different opinions, let us examine them by the light of such evidence as we can fairly bring to bear on the questions at issue. First, then, as to *Adhesion*.—Dead parts will sometimes unite with living parts, and even increase by external addition, like a mineral. In the course of some experiments at Chatham, many years ago, I introduced splints of dead

bone into the medullary canals of the tibiae of living rabbits, and occasionally found that the dead portion had become firmly united to the living bone; while there might be also a deposit of new osseous matter on the surface of the dead bone, and this even when union had altogether failed. In those cases where the dead and living bone had become united, the adhesion was by new osseous matter, so that the two were thus simply cemented together without the aid of any kind of case or clasp formed by the living part. The preparations of these parts are in the museum of the Army Medical Department at Fort Pitt; and as I gave a specimen to the late Mr. Liston, it should be at University College. One of the specimens is depicted in the 3rd fasciculus of drawings from the Army Medical Museum at Chatham, plate x., fig. 13, and the experiments are detailed in the *Medico-Chirurgical Transactions*, 1838. Thus, it may be inferred that union between two parts in the living body is by no means conclusive proof of the life severally of those parts.

"Lightning and Electricity.—In my notes to the Sydenham Society's edition of "Hewson's Works," cases are cited in which coagulation of blood and rigidity of muscles occurred in man and other animals after death by lightning and by electricity. Subsequently a very eminent teacher, having exhorted his pupils to lose no opportunity of making observations on the subject, received a communication from one of them (Mr. Henry Thompson), which Professor Sharpey kindly placed at my disposal, and nothing can be more conclusive of the fact, that the blood is regularly coagulated in sheep killed by lightning. More recently, Dr. Davy (*"Diseases of the Army,"* p. 408-9) examined the body of a man who had met with instantaneous death from lightning at Malta, and though putrefaction was very far advanced, there was rigidity of the muscles, and the heart contained some frothy blood, and a little soft coagulum.

"Hunting and Fighting.—In all my examinations (*Edin. Med. and Surg. Journal*, Oct., 1848) of the state of the blood and muscles in animals either hunted to death or killed by fighting, not only was the blood found coagulated afterwards, but the rigidity of the muscles often increased and hastened. In stags, foxes, and hares hunted to death, the blood coagulated, and the muscles quickly stiffened. Such, too, was the case in game-cocks worried to death by fighting. In some of these birds the blood was found coagulated within the body in less than thirty minutes after death, nor did it fail to coagulate in any one of them; and in every instance the muscles were firmly contracted within four hours from the time of death. These are facts, to be taken for what they may be worth, which you are as well able to estimate as I can be. If you further inquire of old and observant sportsmen as to the state of foxes run very hard or until they drop down dead, you will most probably be told in such cases, as I have often been, that the animal quickly becomes 'as stiff as a brickbat,' so that he may be held out horizontally by the hind legs.

"It would seem hopeless to attempt to reconcile these with Mr.

Hunter's facts and opinions. Perhaps he had seen animals after the post-mortem rigidity had ceased, as it occurs so quickly in these cases, that any one not familiar with them might be thus deceived. And as to the blood, there might have been delayed coagulability, as in the examples given by Dr. Polli; or in hot weather it might have clotted soon, and become soft and pulpy, or even fluid again, from commencing putrefaction. Or a diffuent gore or cruor might be found in one part, while fibrinous clots were hid and escaped notice in some rather distant situation. And this last I have found to be the fact in more than one case, in which the blood was shown to me as having never coagulated after death in the human body. After all, Mr. Hunter's cases were probably true and exceptional, for it is difficult to suppose that such an acute observer could have been deceived in such a plain set of observations. Still, as we have seen, his remarks on the state of the blood in animals killed by lightning are equally unsatisfactory.

"Coagulation in dead Serum.—As we cannot, therefore, admit the certainty or validity of the facts which he adduces in support of his view, let us see how far it may accord with any other facts available for us in this inquiry. If we admit that coagulation is an act of life, we must also admit that the serum is alive, that it may retain its life for days out of the body, and coagulate afterwards at the temperature of the air, under certain circumstances. I have seen two varieties of serum, which had been kept separately in open glasses for upwards of three days, produce, when mixed together, a soft, trembling clot within six hours, the air at the time being from 49° to 60° ; and the fibrin thus coagulated was not destitute of fibrils.

"Coagulation after Pickling and Freezing.—Further, if coagulation be an act of life—if we adopt this doctrine, how can we refuse to admit also that we are able to preserve that life for an indefinite time by pickling with salt and by congelation by cold? As noted in the 12th and 21st pages of Hewson's works, I have kept blood fluid with saline matter for more than twelve months, after which that blood coagulated when diluted with water; and since the experiments of Hewson have been confirmed and extended by Mr. Hunter and Dr. Davy, it is well known that blood may be repeatedly frozen and thawed, and yet retain the power of coagulating spontaneously. These observations are not, and never were, given as dogmatically conclusive against Mr. Hunter's opinion; but merely to suggest what we must be prepared to accept if we adopt that opinion. They are more favourable to it than to the more modern hypothesis, that coagulation is the death of the blood. Nor do they give any support to Dr. Richardson's tenet.

Coagulation without Emission of Ammonia.—What probability is there that ammonia was retained by the specimens of serum as long as they were kept separate, and given out to cause coagulation after they were mixed together? No evolution or discharge of ammonia would make them coagulate while unmixed, nor, indeed, will they coagulate at all while separate, under any circumstances, at the temperature of the air, as I proved by numberless comparative trials. Again, after the blood has been kept fluid by salt for

months, what likelihood is there that the addition of a little pure water produces coagulation by driving out ammonia, or indeed any other volatile matter? This alkali was not thought of in those experiments, which were made for a very different purpose many years since. But in some blood which had been kept fluid by salt in an open jar for thirteen days no emission of ammonia could be detected by the most careful examination from the time of commencing coagulation to its completion; and repeated trials of the like kind gave the same negative results. Dr. Davy, too, before I had experimented with a special view to this subject, had come to the conclusion that the escape of ammonia is not the cause of the coagulation of the blood, and, indeed, that it is not caused by the escape of any volatile matter at all. His first experiments were made on the blood of fowls, and repeated since with the same result on the blood of the pig. Some observations which I made last summer (*Ann. Nat. Hist.*, March, 1862) on the spontaneously coagulable juices of plants were to the same effect. No trace of ammonia could be detected even when the coagulation took place at a temperature of 80°, nor under any circumstances whatever in the fresh coagulating juices, and much less evaporating from them. These are examples of the obstructions which lie in the way of this chemical view, and which will have to be removed before Dr. Richardson can expect it to be unconditionally admitted.

Fluidity of Fibrin.—After all, we might as well inquire the cause of the fluidity of the blood; or, with Mr. Hunter, ask, why it does not coagulate while circulating in the body? Probably, it never is regularly very long fluid. The wants of the economy may appropriate the fibrin not long after its production; and thus the constant formation and assimilation of fibrin may keep up the balance so nicely as to preserve the fluidity of the blood, and so this fluidity would necessarily be lost when removed from the conditions by which it was maintained. The blood never can be the same in distant, or even in comparatively near parts of its course. We know that successive small portions of blood from the very same orifice of one vein may possess different qualities. And how wonderfully must the blood be incessantly changing as it gives out the multitudinous materials for growth or nutrition, and receives additions by absorption, either from the chylæ, lymph, air, or effete parts of the frame. And with what wonderful rapidity and abundance fibrin is incessantly being produced in the living blood, and used in the animal economy, may be inferred from the fact that all the fibrin disappears from the hepatic and renal blood after every passage of that blood through the liver and kidneys.”

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